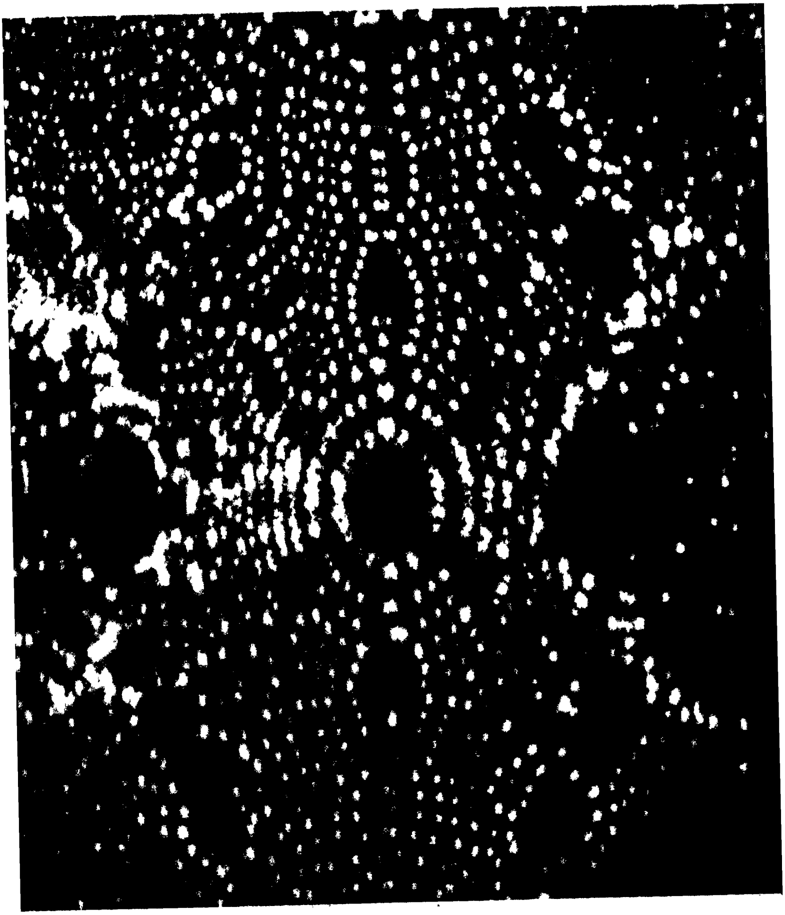

“It would be difficult to imagine an exposition reaching the profoundest questions of modern science mc

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Toward the end of the last century, scientists were certain that they had discovered, identified, and explained all of the basic "laws" and characteristics of the physical universe. Dividing the world into material substances and electromagnetic waves — "matter and motion" — they confidently drew a picture of the cosmos that seemed consistent and complete. When, in 1899, the German physicist Max Planck proposed a radically new explanation for the absorption of light, the sharply defined lines between particles and radiation were blurred and modern physics came into being. This new aspect of physics, called quantum theory or wave mechanics, generated a revolution in man's manipulation of nature and an immense impact on our understanding of the fundamental reality of the world about us.

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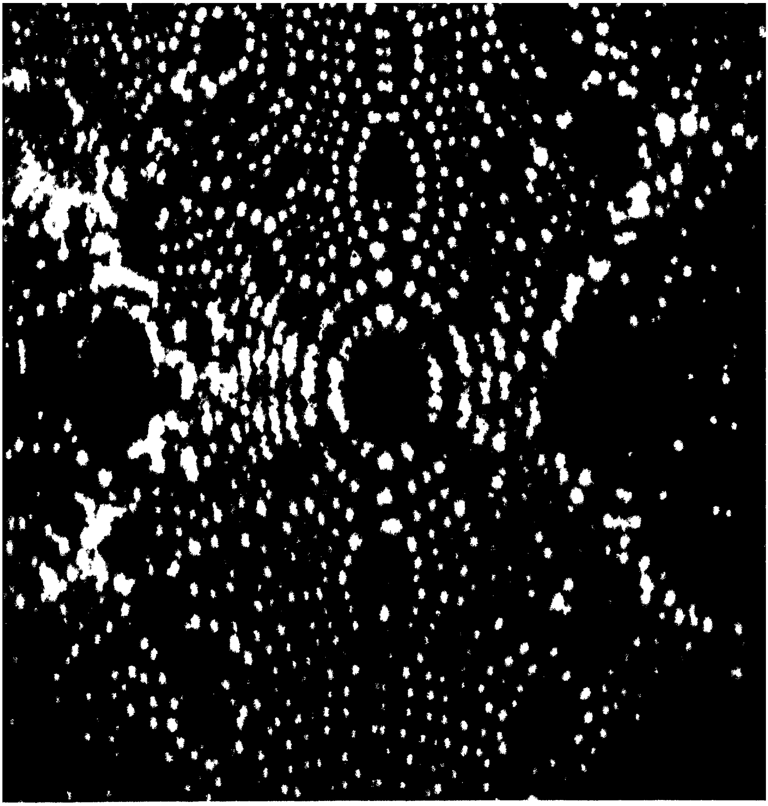
Quanta and Reality



The atoms on the surface of a crystal of rhenium metal, as revealed by the Field Ion Microscope, which gives here a magnification of 2,000,000 (Müller). (*Taken from the front cover of 'Journal of Applied Physics' for January, 1957*)

Quanta and Reality

A Symposium

AMERICAN RESEARCH COUNCIL

© Professor David Bohm, Professor N. R. Hanson,
Dr. Mary B. Hesse, Professor Nicholas Kemmer,
Professor A. B. Pippard, Professor Maurice Pryce,
and Dr. Stephen Toulmin 1962

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Preface

Most of the material in this volume was originally cast in the form of a radio series, broadcast in 1961 in the BBC Third Programme. The contributions of Professors Pippard and Kemmer, and of Dr Hesse, and the discussion were first broadcast in March: they were later repeated as a group on 14th July. Professor Hanson's postscript was first broadcast on 21st August.

The primary aim of the series was to take the layman's recurrent question—'But what are electrons really like?'—and to press it as far as it would go: on the one hand, to investigate how far such a question is inappropriate in the light of modern physical knowledge; on the other, to inquire into the nature of the electronic Emperor's Clothes (if any). The inquiry was never intended to be exhaustive. It offers, instead, a selective glimpse of the bizarre world of the quantum physicist.

In the event, the series took to itself a number of secondary aims. The philosophical issues raised by a simple question led on to practical issues of scientific strategy. The reader may judge for himself whether it was any the worse for these unforeseen implications.

DAVID EDGE

Producer, BBC Science Unit (Sound)

Introduction

BY

STEPHEN TOULMIN

How frequently must our theories about the structure of matter be revolutionized? How soon after we have accepted one new and fine-grained picture of Nature shall we be forced to replace it by another, with still finer discrimination? Will this happen every 200 years, once a century, after only fifty years?... Or may we reasonably hope that, at a certain stage, microphysics will give us a truly *fundamental* picture of Nature, reaching a level of analysis behind which we need not seek to penetrate—the modern counterpart of those ultimate material units of which natural philosophers first dreamed nearly 2,500 years ago? Have we, perhaps, *already reached* this ultimate level of analysis, in the contemporary theories of Quantum Mechanics? Or should we, rather, prepare ourselves for future revolutions in microphysics as radical as those of the past?

These are the broad questions underlying the controversy with which the following chapters are concerned. At first glance they may appear to be purely domestic questions, for theoretical physicists to settle by the professional techniques of their own craft. Up to a point, this is no doubt so: as a matter of *tactics*, every physicist must decide what researches to pursue according to his own professional conscience. Yet this is not the whole story. The debate sparked off by the work of Professor David Bohm (who contributes to Chapter

4 below) raises also important questions of intellectual *strategy*, which carry theoretical physics back into the realm of 'natural philosophy' from which it began. For Bohm challenges any claim that quantum mechanics gives us a final picture of Nature: on the contrary, he declares, the time has come to carry our fundamental theories and concepts beyond quantum mechanics to a new level of discrimination. A whole new range of 'sub-quantum' ideas and entities must be accepted, which will be as different from those of orthodox quantum mechanics as they themselves were from the atoms and waves of classical nineteenth-century physics.

Most of Bohm's professional colleagues have been deaf to this call for a fundamental change of strategy. Some, like Professor Maurice Pryce (who debates the issue with Bohm in Chapter 4 below), have condemned Bohm's proposals as positively misguided and misleading. And, since Bohm's challenge questions the standing of quantum mechanics as a whole, the resulting debate has branched far and wide. It has not only provoked a reappraisal of the intellectual foundations on which the orthodox interpretations of quantum mechanics are based—which are lucidly explained by Professor A. B. Pippard and Professor N. Kemmer in Chapters 1 and 2—it has also precipitated arguments about historical precedents, logical proprieties and philosophical methods. So it is fitting and necessary that, in the present book, two authorities on the history and logic of science (Dr Mary B. Hesse and Professor N. R. Hanson) should enter the ring along with the theoretical physicists.

In order to set the scene for the debate, it will be useful to sketch the historical background against which it takes place. This is desirable for two separate reasons. In the first place, readers who are not themselves physicists will need to be told (or reminded) about the historical origins of quantum mechanics. But there is another consideration also: in trying to decide at the present time whether Bohm's criticisms of orthodox quantum theory are likely to justify themselves, one must rely in part on the exercise of historical judgement. We know from the history of science how men have sometimes hinted at the shape of theoretical 'things-to-come', long before evidence was to hand which could justify their hints: notably, when William Prout speculated, as early as 1815, that all the different atoms of chemistry might consist of a common sub-atomic matter akin to hydrogen. But we also know that time has buried in unmarked graves the speculations of many other men, who have enjoyed no such posthumous vindication as Prout. And perhaps we can state the question before the jury in Bohm's case as follows: 'Are his ideas about a future sub-quantum microphysics based only on hunches and guesswork—like Prout's hypothesis? Or can they even now serve as the starting-point for a genuine intellectual breakthrough—as the speculations of Bohr and de Broglie did for the theories of quantum mechanics?'

The Origins of Quantum Theory

The fundamental theories of nineteenth-century physical science (still known today by the name of 'classical' physics) divided the agencies of the physical world into two sharply opposed categories. On the one hand, there were *material substances*. These comprised the ninety-odd chemical elements, each having atoms of its own distinct unchangeable shape, and also the various compounds whose molecules were formed by the combination of these atoms. On the other hand, there were various species of *fields and radiation*. This class embraced light, radiant heat, electricity and magnetism;

as a result of the work of James Clerk Maxwell, all of these were shown to be transmitted across space in the form of electro-magnetic waves which differed only in their frequencies and wavelengths. Radio waves, first observed by Hertz and later exploited by Marconi, are artificial waves of a similar kind, having a lower frequency—and a longer wavelength—than those of radiant heat.

At the beginning of the nineteenth century the absolute distinction between material atoms (which alone possessed mass) and the less tangible electro-magnetic agencies (which could carry energy but apparently had no mass) was neither obvious nor universally accepted. But, as the decades passed, one discovery after another added to its plausibility, so that by 1875 it was acquiring the status of a physical axiom. Between 1800 and 1810 atomism had been converted by John Dalton from a philosophical system into a positive theory of chemical combination, and by 1860 Maxwell had extended its scope into physics, using it to explain the expansion of gases and the laws of heat conduction and exchange. For a while, it seemed as though the whole subject-matter of physical science could be summed up—to use Maxwell's own phrase—as Matter and Motion. True, the atoms did not interact with one another solely in mechanical ways—by colliding, meshing together and so on. They could also exert forces and transfer energy through the medium of electric and magnetic 'fields of force'. But these fields were still thought of as a secondary aspect of Nature: as their intellectual ideal, theoretical physicists always kept in mind the complete mechanical causality of a well-constructed clock. And until the very end of the nineteenth century there was nothing to suggest how completely, during the next thirty years, the fundamental 'classical' distinction—between the individual, massive atoms of matter and the smooth, energycarrying waves of electro-magnetism—was to be demolished.

The first breaks in the classical line took place on two separate fronts, and the resulting intellectual thrusts did not link up until 1925. During the 1890's J. J. Thomson had demonstrated that all chemical substances contain within them electrically-charged particles having a mass less than 1/1,000th of the mass of the lightest atom—i.e. 'electrons'. Meanwhile, the Curies were demonstrating that the atoms of certain 'radioactive' substances were not immutable: they were in fact gradually transforming themselves into atoms of lead. Working from these starting-points, Ernest Rutherford and his pupil Niels Bohr were already building up, before 1914, a first picture of sub-atomic structure. (This is the picture still referred to as the 'Rutherford-Bohr planetary atom', in which individual negatively charged electrons travel in sharply defined orbits around a compact central nucleus which has a positive charge equal to the 'atomic number' of the particular element.) On this front, the need for a complete new system of 'quantum physics' was not immediately apparent. Elsewhere, however, Albert Einstein had made a crucial advance as early as 1905. Following up Max Planck's suggestion that material bodies *exchanged* electro-magnetic energy only in unit packets or 'quanta'—the size of the 'quantum' being proportional to the frequency of the energy concerned—Einstein now produced evidence that such energy in fact *existed* only in discrete energy-packets: these he christened 'photons'. The phenomena of interference and diffraction made the wave-properties of light undeniable, yet for certain purposes one was compelled (he concluded) to think of radiation in terms of an alternative image—as a rain of electro-magnetic *corpuscles*. And this photon theory in due course dovetailed with the ideas of Rutherford and Bohr, since it appeared that, whenever an atom absorbed or emitted a photon, an electron within the atom 'jumped' from one of its possible orbits to another.

In this way, one leg of the fundamental nineteenth-century distinction had become shaky. Instead of matter alone being composed of discrete corpuscles—all radiation being smooth and continuous—there now turned out to be something 'particulate' about radiation also. And in the years after 1918 the other leg of the distinction also began to collapse. Through the work of Louis de Broglie it seemed that material particles such as electrons might have certain wave-like characteristics, quite as much as photons; and this was put beyond doubt in 1927, when it was demonstrated that a beam of high-speed electrons passed through a thin sheet of metallic foil spread into a diffraction pattern exactly comparable to that produced by a corresponding beam of X-rays, i.e. high-energy photons (see Plate 3).

In order to cope with this rapidly-changing intellectual situation, theoretical physicists between 1900 and 1925 proceeded in something of a hand-to-mouth way, progressively extending and adapting the ideas of classical physics as occasion demanded. Between 1925 and 1930, however, the foundations of a brand-new system of ideas were laid, notably by Schrödinger, Heisenberg and Dirac. This is the theory of Quantum Mechanics, certain of whose fundamental doctrines Professor Pippard and Professor Kemmer expound later in this book. This new system of physical theory rejected two central axioms of the classical system: (i) It treated the basic units of matter and electro-magnetic energy alike as 'wavicles'—i.e. as transferring mass and/or energy in packets of minimum size (quanta), but as interacting and being propagated like systems of waves. (Professor Pippard's account of the ways in which atomic nuclei can 'resonate', using an analogy familiar to us from music, is a particularly lucid explanation of this aspect of quantum mechanics.) (ii) It required one to abandon the 'clockwork' picture of Nature which had been the classical physicists' ideal of explanation. Given the precise mass, position and speed of every atom in a given system at one moment (classical physicists had assumed), one should be able to calculate, in principle, the exact future positions and velocities of all those same atoms. But at the sub-atomic level (it now appeared) the corresponding calculations could not be performed. Using quantum-mechanical principles, questions about the future behaviour of physical systems could be answered only statistically. One could calculate (e.g.) the *average* rate at which nuclei of some radioactive substance would disintegrate, but not the precise moment at which one particular—or even the *next*—nucleus would break up, nor the exact number which would disintegrate during any chosen second of time. (Strictly speaking, this was not a feature of the sub-atomic world only: the same was true of larger-scale bodies also, but with these larger systems the statistical variations were in most cases imperceptible.) A central place in the theories of quantum mechanics was, in fact, occupied by Heisenberg's Principle of Indeterminacy. As a result, the rigid causality of the classical system has been set aside—as Professor Kemmer explains—in favour of ideas about *probability*.

Different physicists reacted to the new theories in different ways. To most of Heisenberg's younger contemporaries, the abandonment of classical causality seemed a small price to pay for the great extension of understanding that followed from the development of quantum mechanics. Yet, as von Neumann soon showed, the price was inescapable. Either one accepted quantum mechanics—indeterminacy and all—as one's fundamental theory of Nature, or one did not. Any attempt to add more detail to the new quantum-mechanical picture in the hope of restoring the older 'clockwork' causality would land one in contradictions. And many physicists, led by Heisenberg, went further; in their view, the quantum-mechanical picture, if sound, was not only fundamental but *final*. In the macroscopic world, all statistical statements (e.g. about 'suicide-waves') report the overall aggregates or averages of large numbers of individual events (in our example, suicides); but the microphysical statements of quantum mechanics were statistical in a more absolute sense. There was, it appeared, no way of reinterpreting these statements as overall aggregates or averages over corresponding large numbers of 'sub-quantum' events. For the limits which quantum mechanics placed on our techniques of observation actually prevented us from giving any *meaning*—in terms of physical observations—to statements about these supposed sub-quantum events. Some physicists, indeed, have even argued that physical theory can concern itself only with mathematical relations between observable quantities, and that the desire to set up a sub-quantum theory is only a by-product of misleading intellectual models. (Professor Pryce defends a similar view later in this book.) But others question, on logical and historical grounds, whether it is either legitimate or desirable to banish intellectual models from fundamental physical theory entirely, in favour of purely-computative mathematical theories. (Dr Hesse argues this case in her essay.)

The Heresies of David Bohm

To begin with, the legitimacy of quantum mechanics as a fundamental theory of Nature was seriously challenged only by a few physicists of the older generation. The most distinguished of the unconverted was Albert Einstein, who tried throughout his later years to find a way of evading the conclusions of Heisenberg and von Neumann. There must (he argued) be *some* means of giving a meaning to statements about the quantum world more precise than the Principle of Indeterminacy seemingly permitted. The resulting controversy took place partly in technical journals of theoretical physics—where the banner of the new quantum mechanics was several times defended with great skill by Professor Pryce—and partly in more popular or philosophical contexts: e.g. in the symposium *Albert Einstein, Philosopher-Scientist*, published in 'The Library of Living Philosophers', Vol VII. By 1939 most of the younger theoretical physicists were convinced that Einstein's objections sprang only from nostalgia for the apparent certainties of nineteenth-century physics; and they were encouraged in this conclusion by the histrionic character of his own dicta—e.g. 'God may be sophisticated, but He is not malicious' and 'I cannot believe that God plays with dice'.

Since 1945, however, a few physicists have once again begun to criticize orthodox quantum mechanics in a way not entirely unlike Einstein's, and (to begin with) many supporters of the orthodox theory regarded this development as purely perverse. The most persistent, profound and controversial of these critics has been David Bohm. (His ideas are argued at greater length in his own book, *Causality and Chance in Modern Physics*, 1957.) Four strands in Bohm's argument must be distinguished. First, there is his general claim: that theoretical speculation about sub-quantum physics is—for all that von Neumann may have proved—both legitimate and necessary. This general position he defends not only with physical arguments but also with historical and philosophical ones, to which we must return shortly. In the second place, Bohm argues, despite all Einstein's failures, ways can be found of giving a meaning to microphysical statements more precise than the Principle of Indeterminacy acknowledges. (Though one cannot measure both the position and momentum of any *one* sub-atomic particle with unlimited precision—since, in measuring either of these variables, we unavoidably affect the other—we might nevertheless make an exact measurement of—say—the energy of a given particle, and also determine exactly the time of this measurement, using a clock-system in which the particle under observation was not involved.) In particular, Bohm believes, alternative theories about the sub-quantum world will give observably different results, particularly when one is concerned with very high energies or with the internal structure of the atomic nucleus.

Further, in presenting his own outline of a possible sub-quantum theory, Bohm employs explanatory models of the sort Heisenberg had rejected. The 'wavicles' of the quantum world he compares to clouds or tidal waves: they represent, for him, transient configurations with blurred edges, continually forming, dissolving and travelling across an underlying sub-stratum (or 'field') of energy. On this particular model, the probabilities and statistics of orthodox quantum theory can once again be treated as probabilities and statistics of a familiar kind, i.e. as statements about the average outcome of numerous turbulences in the sub-quantum energy-field. Finally, Bohm suspects that the next major step forward in theoretical physics will compel us to reconsider certain assumptions on which all physical theories have rested ever since the seventeenth century. At that time, René Descartes established the practice of analysing problems in physics by the mathematical techniques of 'co-ordinate geometry' (which is sometimes called Cartesian geometry, in honour of its founder). In Bohm's view, the use of Cartesian geometry in physics involves accepting uncriticized assumptions which it is now time to abandon. And, just as Einstein based his arguments for the Theory of Relativity on the rejection of some uncriticized Newtonian assumptions (about the concepts of 'time' and 'simultaneity'), so Bohm looks forward to a new system of microphysics which will reject corresponding Cartesian assumptions about space and geometry. In place of the long-familiar techniques of co-ordinate geometry, he is experimenting with the redefinition of spatial concepts in terms drawn from another branch of mathematics—namely, topology.

To some extent, all four of Professor Bohm's claims involve considerations of a highly technical kind. To that extent—as Professor Hanson argues—the theoretical physicists must be left to fight out the issues among themselves. This is particularly true of the three more detailed claims. Although one can argue on general grounds that *some* sub-quantum speculations are legitimate and necessary, the precise form of a satisfactory sub-quantum theory (if one were possible) can be settled only by building up such a theory in detail; and this theory must provide a system of physical ideas which explains the facts of Nature more successfully than existing quantum theory. Astronomers in the Ptolemaic tradition were left unmoved by Copernicus' first excursions into astronomy. Before paying much attention to his general principles, they waited for a demonstration that the new system had mathematical capabilities at least equal to those of the established theories. This is not to imply that David Bohm is playing Copernicus to Heisenberg's Ptolemy. On the contrary, the title of 'a modern Copernicus' is one which Bohm will have to *earn*, by demonstrating the superiority of his proposed new system in detail. Unless he succeeds in doing this, orthodox quantum physicists, like

Professor Pryce, will continue to regard his criticisms of quantum mechanics as backward-looking—as a revival of Einstein's attempted counter-revolution—rather than as being 'the shape of things to come'.

The Evidence of History

Yet there is one issue under dispute which can be effectively discussed—even if not wholly resolved—in more general terms. For the strategic question remains: Has the time come to reconsider the legitimacy—even the desirability—of building up some new 'sub-quantum' theory?

This problem of strategy involves one in questions of history as well as of physics. David Bohm of course accepts von Neumann's demonstration, that one cannot restore classical causality by *adding to* the principles of quantum mechanics. As he makes clear, the theory he wants to see developed would *replace,* not supplement, existing quantum mechanics, particularly at the highest energies and the finest levels of discrimination. And one cannot estimate the likelihood of some such theory justifying itself merely on the basis of arguments within existing quantum theory. This question carries us beyond the codified principles of current theoretical physics into a realm of 'case-law', and we are forced back on to historical precedents.

On this plane of argument, some of the signposts certainly do point in Bohm's general direction. If we ask how soon in the nineteenth century there was any serious justification for questioning the foundations of Dalton's and Maxwell's classical theories, one thing is immediately clear: that, between 1815 and the 1880's, the whole situation was transformed. When William Prout threw out the first hint that different chemical atoms might be formed from common subatomic units, he did so too soon. At that time the proper strategy for physical science was to consolidate the new atomic picture of matter which Dalton had introduced only a few years before. Until this had been done, speculation about possible sub-atomic structures could yield no real profit. By 1880, however, the very success of the classical picture had created a new state of affairs, and one could at last begin to put down new soundings. With his Periodic Table, Mendeléeff had shown that chemical atoms fell into well-defined families, having resemblances which could not be explained on Daltonian principles alone. Meanwhile, in the theory of radiation, mathematical calculations were leading to paradoxical results which were to be eliminated only by the quantum theory. Even so, the idea of reviving Prout's speculations about a sub-atomic world encountered great resistance; and when J. J. Thomson presented his first report on the discovery of electrons some of his colleagues thought the whole thing must be a practical joke.

Now it is arguable that, since 1926, the very success of quantum mechanics has transformed its standing in a similar way. Whereas at that time physicists knew of only two sub-atomic particles, at least thirty such particles have by now been demonstrated, the proton and the electron being only the commonest and longest lived. Once again, these thirty-odd 'fundamental particles' fall into well-defined families, having similarities and differences which must eventually be explained. The question then is: Will the story of classical physics and chemistry be repeated? If so, these similarities will find their explanation only at a sub-quantum level. And the mathematical anomalies appearing in certain branches of quantum mechanics prompt a similar reflection; for up to now these have been dealt with only by arbitrary mathematical procedures, such as the 'renormalization' to which Professor Hanson refers.

Unfortunately history is like the Delphic Oracle. Its prophecies are always ambiguous. It is certainly *more* reasonable to speculate about a possible sub-quantum world in the 1960's than it was in 1926; but whether all the historical signposts are pointing in this direction is another question. Perhaps the proper attitude to adopt towards history—as towards any other oracle—is to treat it as a source of warnings and challenges. The multiplication of the fundamental particles, and the mathematical necessity of renormalization, can then serve as warnings to conservatives that existing quantum theory cannot—as it now stands—represent in every detail the last word in physical theory. They also present a challenge to radicals, such as David Bohm, to demonstrate the merits of their ideas in the only way their colleagues will find unanswerable.

Perhaps all the outstanding problems of microphysics will be solved without modifying, or going behind, the principles of orthodox quantum mechanics. Perhaps they will not. Either way, the problems themselves remain a standing intellectual challenge. Whichever way the solution eventually comes, what Professor Hanson says is past question. The men who first surmount these intellectual obstacles will find that 'the physicists of this world will be at their feet; science will have ascended to a new plane of power and fertility'. Judged by the most enduring values of science, this intellectual contest is far more significant than any technological race to be first on the moon. Those of us who are not ourselves equipped to contribute to the debate can only wait and see which way the verdict will go. But, where such profound questions of natural philosophy are at stake, all of us should take an informed interest in the questions at issue, and form our own judgements about the progress of the argument. If the essays which follow help to make this possible, the chief purpose of this book will have been well served.

I

Particles and Waves

I

Particles and Waves

BY

A. B. PIPPARD, F.R.S.
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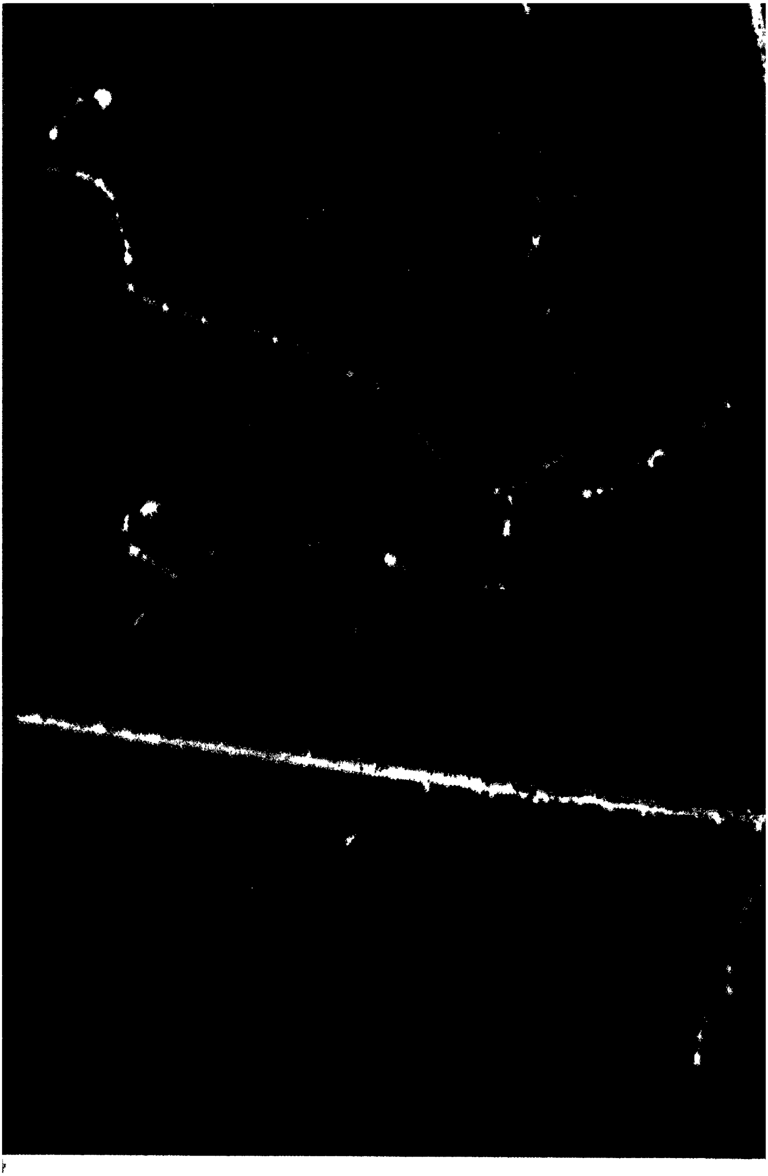
TO ALL those who have watched, perhaps apprehensively, the exaltation of the physicist into a figure of power, it may come as a surprise when I say that at a more philosophic level the last thirty years have seen him forsake his self-appointed status of near-divinity. He now admits himself a mere man, with all a man's limitations and ignorances. To the great figures of the past, the physical world was something from which they could detach themselves in imagination; having made certain observations on the celestial bodies they could retire to their studies, emerging years later to predict where a new planet was to be found—and sure enough there it was. There was no call to doubt that the configuration of material objects, whether planets or atoms, changed steadily from moment to moment, or that in principle the whole glorious pageant was open to the inspection of any who cared to look. From his seat among the gods the physicist observed the play, confident that if he did not interrupt too rudely his presence would not affect the players. Nowadays, he is not so sure when it comes to the things that happen on the atomic scale. Certainly the planets still seem to take remarkably little notice of human intervention, but the atoms are more sensitive. Still more so the electrons, the lightest of the constituents of an atom. Apparently even the most delicate inquiry into their state of being upsets them so much that their future conduct is materially altered.

Let me illustrate this point by describing an experiment that can be done, with some difficulty, in an electron microscope. If the beam of electrons emitted by the filament is interrupted by a plate with a small hole in it, a vague splash of light appears on the screen, and can be photographed. It is caused by the electrons coming through the hole and spreading out beyond it. If now we make another hole very close to the first, we don't see two overlapping splashes of light, but a single splash which is crossed by a set of parallel lines of alternating darkness and light. At the brightest point the image is four times as bright as when there was only one hole; at its darkest, it is quite dark, and the average brightness is twice what it was, as you would expect if two holes let through twice as many electrons as one. Now let's choose one of the brightest spots and try to answer the question: Which hole do the electrons come through that arrive at this spot? We might see what happens when we cover up one of the holes: immediately the dark and bright lines vanish and the intensity at the point we are studying is quartered. But that doesn't mean that three-quarters of the electrons were going through the hole we covered, for exactly the same would have happened if we'd covered the other. The fact is that the pattern of lines we observe only occurs when both holes are open, and under such circumstances that we cannot decide which hole any particular electron went through. So here is a case where the result of an experiment is markedly affected by any attempt to follow it in detail. Between the moment when the electron leaves the filament and the moment when it arrives at the screen we must deny ourselves the luxury of looking at it.

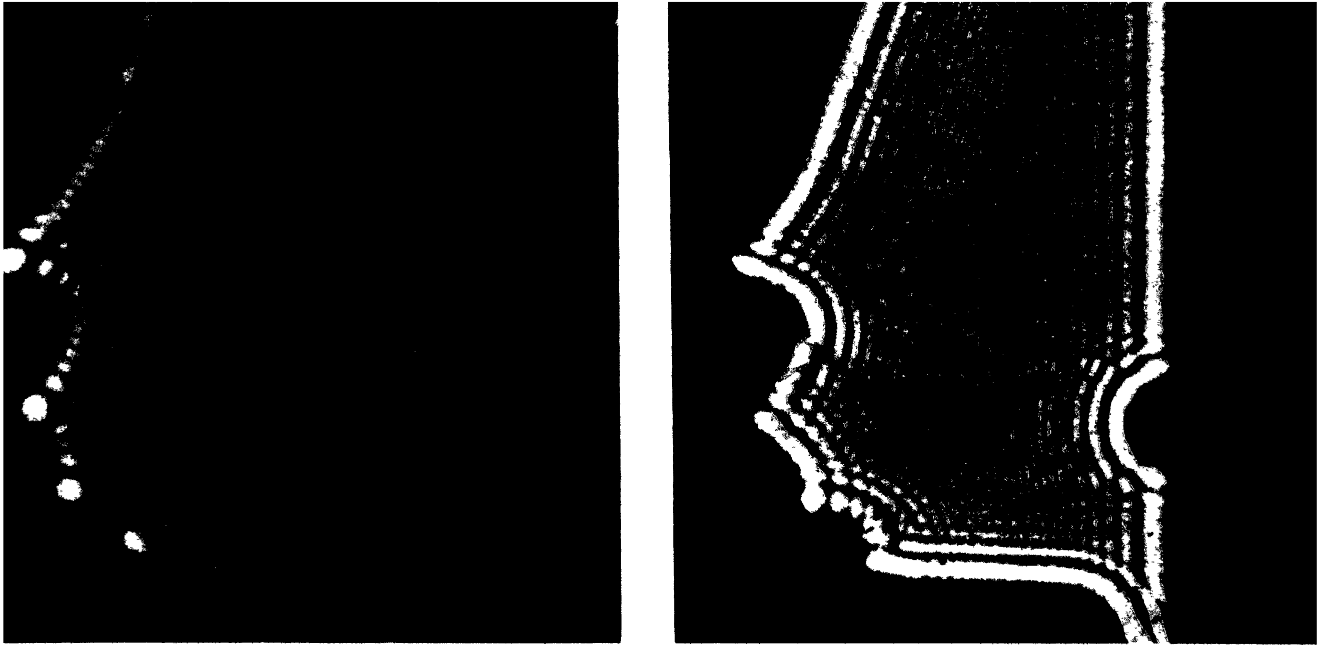
Does this mean that there is no sense in asking what the electron is actually doing? Most physicists and philosophers would agree in saying that you must not ask questions if you know it is impossible to find the answer. But although this is the orthodox attitude, not everyone will accept it. However, the usual attitude among working physicists may be put in this way: since physics is concerned with understanding the observable world, it is enough to devise a system of thought (in other words, a theory) which correlates different observations, and which allows future observations to be predicted from present knowledge. It is sufficient that the technique of prediction should work; it need not imply any explanation of the connection between cause and effect. If we like to picture to ourselves some mechanical model which satisfies our craving for an explanation, that is our private affair. Maxwell was not ashamed to invent models of the aether, and indeed found them helpful to his thought. I doubt if he ever really believed them, and certainly by the time he had perfected his theory the models had served their purpose and been eliminated.

For the experiment with the electrons and the two holes, the appropriate theoretical technique for predicting where the electrons will hit the screen is known as Wave Mechanics; for the simple reason that the mathematics involved is very like the mathematics which is used to solve problems of wave motion. If you have studied optics you will recognize this experiment as Young's experiment performed with electrons instead of light. Thomas Young found, about 1800, that when light was passed through two pinholes in a screen, and allowed to fall on another screen, what you saw on the screen was a set of parallel bands, interference fringes, so called because they arise from the mutual interference of the light waves which come through the two holes. It was this experiment that was one of the decisive pieces of evidence for the wave-nature of light. When we see electrons, and indeed other atomic particles, behaving similarly, we are bound to wonder whether what we have got into the habit of calling particles may not really be waves. Yet there was good reason for the habit. The tracks seen in a cloud chamber (Plate 2) are very like what one would expect to see if a tiny bullet were fired through. Still more convincing are the clicks in a Geiger counter, each one announcing the arrival of something sharply defined, discrete and indivisible—in fact, a particle. But I must stress that the use of these terms does not imply that electrons or protons are really particles, or really waves, or really anything at all. As I use them here the words are merely picturesque flourishes employed as a shorthand notation to indicate the sort of observation or mathematical process I am referring to.

But it is no part of my task now to elaborate the philosophical implications. What I want to concentrate on is the description of experimental observations which compel us to believe that on the atomic scale the motion of particles is as if they were guided by waves. The example of Young's experiment is only one of dozens in which the behaviour of electrons and other particles passing through screens or round obstacles or being scattered by the atoms of a crystal can be exactly matched by the behaviour of light waves or X-rays under similar conditions (Plate 3). As a matter of fact, this typical wave behaviour could have been predicted from the earlier quantum theory of Bohr, which was the first really systematic attempt to reconcile the old ideas with the new facts. Bohr's theory may be summarized by saying that it allowed the particles to obey the rules of classical mechanics,

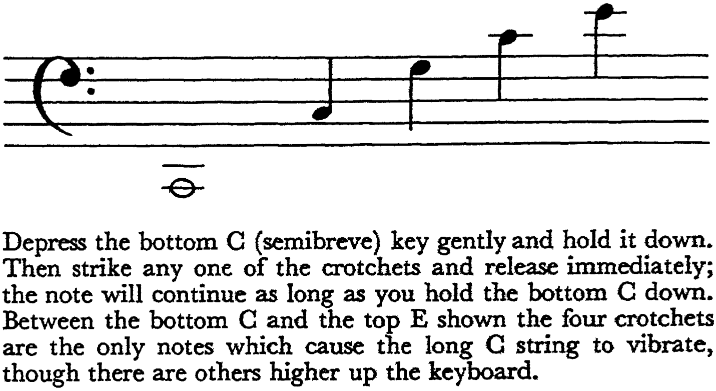


The electron as a particle. Cloud chamber photograph of electrons following wavy paths as they pass through a gas, bouncing off the atoms. The faint tracks are due to electrons, the dense tracks to the much heavier α-particles which are less readily deflected by collisions. (*Taken from 'Radiations from Radioactive Substances': Rutherford, Chadwick and Ellis*, Cambridge, 1930)



electron as a wave. The left-hand picture was formed by electrons emitted from a sharp point and passing through an apertu
een tiny opaque crystals. The bands round the edges of the shadow are diffraction fringes typical of wave motion, as can
from the invitation of the effect in the right-hand picture, which is the shadow formed by light passing through a hole in
metal sheet. (*Taken from a Japanese catalogue of electron microscopes: photograph specially made in the Cavendish Laboratory*)

but imposed additional restrictions so that out of an infinite variety of paths only certain special ones were permitted. These new restrictions operated rather like the offside rule in football: it's not quite obvious why it is put in, but it makes the game go better. Bohr devised his theory to account for the fact that the electrons in atoms can have only certain energies: that is, they are quantized; but it also implies (as was not realized at the time the experiments were done) that electrons would be scattered by crystals into certain special directions only. Still, compared with Bohr's idea, the wave picture gives a much more natural account of this effect, and it is not surprising to find that it also accounts naturally for the quantization of energy in atoms, for something very like this is a well-known property of waves. You can illustrate this on the piano. A very long (ideally, infinitely long) stretched string can carry waves of any wavelength, but if the length is curtailed by fixing both ends firmly, as in a piano, it can vibrate only at those frequencies which allow the wave to fit neatly into the length of the string. So if you strike a low C the note you hear is the lowest frequency allowed. You can



Depress the bottom C (semibreve) key gently and hold it down. Then strike any one of the crotchets and release immediately; the note will continue as long as you hold the bottom C down. Between the bottom C and the top E shown the four crotchets are the only notes which cause the long C string to vibrate, though there are others higher up the keyboard.

make the string vibrate at twice its fundamental frequency by depressing the key silently (this simply removes the damper) and then briefly striking the note an octave above it. So, too, you can make it vibrate at three times by striking a G, or four times by striking the next higher C, and so on. But if you try to make it vibrate at any intermediate frequency —by striking an $\mathrm{E}^{\flat}$, say—nothing happens. The quantization of energy in an atom follows in very much the same way from the wavelike character of the electron. If the electron is to be confined within the small compass of the atom it must have the right wavelength to fit in, and this means the right energy.

So far I have dealt with phenomena in which the wave explanation comes as an improvement on what was not beyond the capacity of the earlier theories, but I must now describe in some detail what I regard as the most compelling single piece of evidence in favour of the wave theory, for it is outside the range of Bohr's theory; this evidence comes from the radioactive decay of heavy nuclei—uranium, radium and others.

There is one common mode of decay in which the nucleus emits an alpha-particle, which is the nucleus of helium and is made up of two protons and two neutrons. Each species of nucleus which indulges in this activity ejects alpha-particles with a definite energy; this, by the way, is a quite unclassical sort of thing to happen, for it shows the existence of fixed energy states inside nuclei. But that is not our immediate concern. To explain the special problem set by alpha-decay, I must ask you to think of the forces which hold a nucleus together. Some positive force is needed, for the constituent particles are either neutrons which do not influence one another electrically, or protons which repel one another. But there are attractive forces as well between protons and neutrons which act only at very short distances and which are strong enough to overcome the electrical repulsion and keep the nucleus together. So that we can think of an alpha-particle as held to the rest of the nucleus much as a single molecule is held in a drop of liquid; but if it gets a short step away the attractive forces become ineffective and it then flies off, repelled by the electric forces. Now if we were trying to account for the behaviour in terms of classical dynamics, we should say that the emission of an alpha-particle is like the evaporation of a molecule from the liquid. Any molecule which by chance acquires enough energy to break the bonds of the attractive forces can get away. But immediately we run into difficulties. In the case of an evaporating liquid we know that molecules from the vapour can recondense on striking the liquid surface. There is traffic both ways. If we were to take a molecule that had broken through the barrier from liquid to vapour and send it back again at the same speed it would return to the liquid. An entirely analogous experiment can be carried out with alpha-particles. We can shoot them at a target composed only of residual nuclei from alpha-emission. Now not only do they not re-enter the nuclei, but it is clear from the way they are deflected on collision with the nuclei that they have no chance of getting in. They are brought to rest by the repulsive electric forces and thrown back before they get close enough to be within range of the attractive forces. What this experiment tells us is that the alpha-particles which are emitted by the nuclei manage to break through the barrier of the attractive forces without having enough energy to do so; enough energy, that is, in terms of the laws of classical mechanics. It is as if you free-wheeled on a bicycle from the top of a 100-foot hill, and thereby acquired enough drive to take you over the top of a 200-foot ridge; and, what is even more extraordinary, when you got well down the other side of the ridge, it is as if you were still moving only as fast as if you'd started 100 feet up. In fact, what the alpha-particle seems to do is what you would do on your bicycle if there happened to be a tunnel driven through the ridge. Of course, there is no convenient tunnel for the alpha-particle, but because of this analogy the behaviour is called the 'tunnel effect'. The explanation of this behaviour lies outside the range of Bohr's quantum physics. Classical mechanics allows no possibility of the particle's escape from the nucleus, and so Bohr's theory, which in this context merely adds restrictive conditions to classical mechanics, is bound to reach the same conclusion. It was only with the invention of wave mechanics that the effect became intelligible, and in this framework it was immediately intelligible, for this sort of tunnelling is a very characteristic trick of waves. Radio experts will recognize a similarity here to the piston attenuator which depends for its operation on transmitting energy through a section of tube where strictly no wave can be propagated, and the same effect can be demonstrated, though with difficulty, with light waves. If I arrange a prism so that a light beam inside the glass strikes one surface at too great an angle for it to emerge into the air, total reflection occurs and nothing escapes from the prism. But there is a disturbance in the air close to the glass surface, and if I hold another piece of glass very close I can get light to pass into it, through the intervening air space in which no light ought to be. It is just this wave phenomenon which allows the alpha-particles to pass through the thin forbidden region round the nucleus and escape, and a mathematical working of the problem explains with convincing precision many finer points, such as why the nuclei that emit energetic alpha-particles are so short-lived compared with those which emit only slightly less energetic particles. A factor of two in energy makes a difference of 10^{42} in lifetime . . . an astonishing effect, yet perfectly intelligible in terms of waves.

These, then, are a few experiments which would serve to convince us that electrons and other particles are waves, if only we didn't know that they also possess the one property that is most characteristic of particles and not of waves . . . that of indivisibility. You can take a train of waves and divide it in two, as a sea wave is divided by a headland. But no one ever observed half an electron. Somehow we have got to reconcile the two aspects into one theory, and that is what quantum mechanics does. As a system of calculation which gives the right answers it is overwhelmingly successful. But whether it tells us anything about what matter is actually like is another question.

2

Waves and Probability

2

Waves and Probability

BY

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PROFESSOR PIPPARD has shown that many features of the behaviour of electrons and other sub-microscopic particles make no sense within the framework of what we now call classical physics. By this term we mean the physics of the last century, standing firmly on the twin foundations of Newton's laws, which describe the motion of massive bodies, and Maxwell's laws, which describe the behaviour of the electro-magnetic radiation. In terms of classical physics the observed behaviour of electrons and nuclei appears to be extremely paradoxical. Yet all Professor Pippard has said is based on very firmly established observational evidence. So the modern physicist faces this dilemma. He is confident that his experiments are correct. He also knows beyond any shadow of doubt that the picture he gets from classical physics is logically consistent. What's wrong then? The only answer he has found is that the entities he observes, the things we have called electrons and nuclei and electro-magnetic waves, are in fact not what he thought they were. The so-called particles are not merely different from Newtonian point particles, they are objects of an entirely different category.

They do not satisfy Newton's and Maxwell's laws, and new laws had to be found to describe them. The theory which was first propounded in 1925, Quantum Mechanics, is still generally accepted today. This theory uses words and concepts inherited from classical physics, but which in their new context have acquired subtle shades of meaning. So let me begin with a warning. You must be prepared to accept that, whenever I use a word like electron or nucleus or particle or wave, it is for lack of a better one. I am not referring to an object which has all the properties which your common sense and your knowledge of classical physics leads you to expect. You will see that the quantum particle is not exactly a particle as you know it, nor the wave exactly a wave.

Let us begin by considering the description of a simple particle, the electron. I choose the electron and not some other object because it was the first to have its behaviour studied in great detail. Very much the same things could be said about the proton or the neutron, which today, in nuclear physics, occupy the centre of the stage. What, then, does quantum mechanics say about the electron? In the first place, quantum theory accepts all those facts which in 1897 led J. J. Thomson to announce to the world that negative electricity is composed of extremely small particles. He named them electrons and undoubtedly conceived of them as particles in the full classical sense. And quantum mechanics, when it describes electrons, assigns to them many oi the properties of classical particles. It speaks of such things as the electron's position, its speed, its energy and so on. Each of these properties can be measured just as if you were dealing with a grain of sand or even a billiard ball. For instance, one of the properties that turns out to be particularly important is the product of the electron's velocity and its mass—that is, its momentum. If a billiard ball hits a suitable piece of measuring apparatus it will transfer its momentum to it; in this way its momentum can be measured precisely. In principle, exactly the same can be done with an electron. And we can also, in principle, measure all the electron's other attributes.

Now you might take it to be obvious that if all the measurements I have spoken of can be made, then we ought to be able to collect all these bits of information and piece them together. This would allow us to follow how the electron changes its position from one point to the next, to measure its velocity, find its energy and so on. In other words, we could reconstruct its path through the apparatus. It is true we might find that the path of our electron is not what we expected from the purely classical picture, but then all we would have to do would be to find a better law of force to describe its interaction with its surroundings. This situation would not in itself contradict Newton's laws. But it is here that the essential difference between classical and quantum physics shows up. There is a definite, and surprising, limit to this description of the electron as a particle. It is not in fact possible to do what I have suggested, to piece together the path of an electron, because it turns out that the constituent measurements cannot all be performed simultaneously on a quantum electron. There are, for instance, conditions, such as the two-hole experiment discussed by Professor Pippard, in which we can know a great deal about the momentum of the electron and can make use of what is known for predictions about the future behaviour, but where at the same time we have to swallow the unpleasant fact that we must forgo detailed knowledge of the position of our electron in the experimental set-up. In the two-hole experiment we cannot say that the electron passes through one hole and not the other, and we have, indeed, to allow specifically that it passes through both. This does not contradict anything we can verify, because if we did start verifying, say by closing one of the holes, we would be destroying the diffraction experiment, and as long as both holes remain open there is a definite probability that the electron can be detected passing through either of them. If we cling to the preconceived idea that the electron is a Newtonian particle, all this seems to be sheer raving lunacy.

It is, of course, this contradiction of everyday experience in the behaviour of the electron which has put the quantum theory outside the layman's range. The essence of it is that the properties possessed by an electron at different times, like its position or velocity, are not connected directly; but when we measure these attributes the probabilities of particular results being found are related to each other. Accurate knowledge of position necessarily entails inaccurate knowledge of velocity. Even if our measurement tells us that our electron is here now, we cannot conclude that it will for certain be over there in half a second, because we are bound to be ignorant of its velocity. You might object that we could have measured its velocity just before we measured its position, but the very fact of making a position measurement disturbs the electron to such an extent that any previous knowledge about its velocity becomes useless. In general, the maximum information we can have about the electron at any one time is that its position is one among a whole range of positions, with some knowledge about how probable any position is, and, in the same way, that its velocity is one of a whole range of possible velocities. If these probabilities are known at any one time, we can work out mathematically the probabilities at any future time, but more is impossible. Heisenberg's famous uncertainty relations tell us exactly how great a precision can be reached.

If you are mathematically minded, this 'calculus of probabilities' may satisfy you completely as a substitute for your classical picture of an electron, but it is in the nature of the human mind (and physicists' minds are no different from other people's) to crave for a *picture* of what they study. The simple classical particle picture won't do. So it was a very important step in the history of quantum mechanics when Schrödinger succeeded in casting the probability calculus into a form not quite so austerely mathematical, and in some sense pictorial. He showed that instead of talking of a distribution in space of probabilities for finding a particle, we can think instead of the intensity of a wave. The physical basis of this idea is particularly evident in experiments like the two-hole experiment. In fact, the immediately visible feature of this experiment, the overall pattern on the observation screen or on the photographic plate, looks exactly like the result of the same experiment performed with waves. The particle structure only matters if you want information about each separate electron in the beam.

But the wave description is not only important in particular experiments like this. The uncertainty relations themselves, which fix the limits within which the classical particle description of an electron may be used, become much easier to visualize with the help of the wave language.

Let me try to explain how these two apparently so different physical pictures dovetail into each other. It is well known to anyone working with waves, whether they be sound waves, water waves or electric waves, that a wave of a single, sharply defined wavelength must needs exist throughout the whole of space. When in practice you have waves concentrated in some particular place, these are built up with a whole range of wavelengths combined, the individual wavelets cancelling out everywhere else. In radio terms this means that a transmitter broadcasting only short pulses occupies a larger band-width than the same transmitter broadcasting continuously. So you have the relationship . . . sharp single wavelength—wide extension . . . and on the other hand, limited extension—wide spread of wavelengths. To translate this classical property of waves into the language of quantum mechanics, we make use of the famous relation suggested by Louis de Broglie: that a particle's momentum, and hence its velocity, is inversely proportional to the wavelength. It then follows that precise knowledge of the electron's velocity necessarily goes with wide spread—that is, lack of knowledge of its position—and vice versa.

So we see that if we start by looking at the electron as a particle in the classical sense we run into limitations, but that these limitations can be deduced from a wave picture. The de Broglie-Schrödinger waves come in as a mathematical fiction governing the probability of the particles behaving this way or that. But all this is only one side of the story. There is another side which is in the forefront, for instance, when we are concerned with the behaviour of light. For light is a wave disturbance in the classical view already. The diffraction experiment we have discussed for electrons can be performed for light just as well, but now the classical physicist will insist that the wave is the true underlying reality and is physically something far more concrete than just a way of describing a probability distribution. He would not be wrong in this, even though we know that the energy carried in a light wave interacts with matter in the same way as if light too consisted of a kind of particle, called the photon. I said 'as if' just now, but I would not be wrong on the quantum view if I said that it *is* made of particles, provided I meant by particles no more than when I speak of the quantum electron. Similarly it is by no means the only view possible to say that the electron is a particle that sometimes behaves 'as if' it were a wave. I can get a description of all I observe equally well if I consider the electron wave as the basic thing. This is particularly true at those very high energies when experiment shows that electrons and other particles may be almost as short-lived as photons. But even in the more familiar domains of atomic theory the use of a wave image is sometimes a closer direct description of what we know than a particle picture.

Take the question: 'What does an atom look like if it is left on its own?' I suppose the picture that springs to most people's minds today is the one you find on practically every cover design in popular science literature: a central nuclear spot and around it a lot of looped tracks representing electron orbits. This is the best image we have, if we stick to the particle picture of the electron. But if a scientist really wants to know how atoms fit together to form molecules or crystals, how much space each atom occupies and what patterns the whole structure forms, he will, in fact, not be concerned in the least with the particle picture, but instead will picture the electrons as continuous wave patterns in space. You may be familiar with the patterns of wave vibration that show up if, say, a thin metal plate or a column of air, or a piano string, vibrates—regularly spaced regions of intense vibration side by side with regions of rest. The patterns made by electron waves in atoms are not only similar to these but of the same essence. In understanding such matters it is of no importance to know that in entirely different experimental conditions, in which the whole pattern is necessarily destroyed, we can catch an electron behaving like a small particle.

There is a duality in the nature of all quantum-mechanic entities, whether we call them electrons, for preference, in one case or electro-magnetic waves, for preference, in the other. Niels Bohr has coined the word *complementarity* to describe this situation and has thereby stressed that the circumstances in which one or other view is natural are always quite different, so that a conflict between the two descriptions can never arise.

Just to give one illustration, let me come back to Professor Pippard's description of how an alpha-particle is emitted from a nucleus. The nucleus is unstable; sooner or later the alpha-particle will be shot out. Here the wave picture is very simple: we start off by having a very high wave intensity inside the nucleus and no intensity outside. As time goes on, the wave, so to speak, 'leaks out' so that there is less intensity in the nucleus and correspondingly more spread over the whole of space outside. After a very long time the whole concentration of disturbance in the nucleus disappears. By this time it is virtually certain that the particle has been emitted. We could take this to give a full description of what is happening, but in fact the sort of observations that are made in this situation link up much better with a translation of our picture into the particle language. The question we then ask is: When and where shall we observe the particle? And we must remember that, if we observe it, we drastically interfere with our original system of nucleus plus alpha-particle, to which the wave picture applied only so long as it was not disturbed. So all we are left with is the knowledge that the single observation of the alpha-particle just emerging from the nucleus is most likely to be made at the beginning of our experiment and that the chance of its being seen to emerge later diminishes at a rate that we calculate from the way the wave leaks out. The experimenter working with many nuclei in a piece of matter will relate this result to the numbers of alpha-particles he observes as time goes on.

Here we have a case where, for the theoretician's work, the wave picture is by far the easier to use, but where he has to translate it into the particle picture to give the observer the kind of answer he wants, even though, as Professor Pippard explained, the alpha-particle behaves here in complete defiance of classical laws. This is but one example of how this curious dual scheme of quantum theory works to the physicist's satisfaction. For all its strangeness, we are confident today that it gives us a fuller and deeper account of Nature than did classical physics.

3

Models and Matter

3

Models and Matter

BY

DR MARY B. HESSE
of the University of Cambridge

PROFESSOR PIPPARD reminds us that, at least philosophically speaking, the physicist has recently become a modest man. He and Professor Kemmer have described some of the experiments which could not be fitted into classical physics and some of the new theoretical ideas which have been developed to explain them in quantum physics, but they have not tried to answer, or even to ask, questions about their underlying reality or meaning. Like Galileo when he was asked about the cause of motion under gravity, they would reply: 'That belongs to a higher science than ours.' Now I want to inquire whether these new theoretical ideas are only 'pictorial flourishes', as Professor Pippard called them, only convenient shorthand for talking about a mathematical structure of calculations and predictions, or whether it still makes sense, in some sense, to ask about their significance for our knowledge of the real world. Before we accept too hastily Galileo's view that this inquiry belongs to a higher science, to 'philosophy' or 'metaphysics' rather than to physics, we must look a little more closely at the way physicists actually make use of their language about particles and waves. Do they really use it as we would expect if it were merely shorthand, or a concession to the weakness of non-mathematical minds? And, if not, does their use indicate that after all they are taking it more seriously than they sometimes seem prepared to admit?

But first let us look at a simpler example, where physicists have claimed that they are discovering, or probing into, the microscopic structure of matter. Consider how the relations between pressure, volume and temperature in a gas can be explained by means of a model of small particles. There is no reason to expect that we shall find only one possible model which fits the facts we are trying to explain. Indeed, in this case there were two simple models, both discussed during the seventeenth and eighteenth centuries, and both giving at that time equally good explanations of the behaviour of gases. One was suggested by Robert Boyle. He imagined that the particles of the gas are like little coiled springs, which exert increasing pressure on each other as they are crowded together, and spring apart when the outside pressure is released. Newton's version of this model was simpler and more abstract. He imagined the particles as spheres which exert repulsive forces on each other, inversely proportional to the distance between them, so that when the particles are crowded together they exert greater resistance to the pressure, and are ready to spring apart when the pressure is removed. There is something cleaner and more satisfactory about Newton's ideal spheres than about Boyle's springs, for why should the ultimate particles of matter be like anything so artificial as a spring? But Newton's model is also less easy to picture in terms of bodies familiar to us, because we do not have much experience of ordinary bodies acting on each other in this way across empty space. I shall come back to this feature of Newton's model later on.

Both Boyle and Newton thought of the gas as essentially static, something like a sponge or rubber ball which shows elasticity when compression is removed. A radically different model was suggested some decades later by the Swiss mathematician Bernoulli. He imagined a gas to consist of a large number of particles in rapid random motion in all directions inside the containing vessel. There is no need now to suppose any forces acting between the particles except their collisions with each other and with the walls of the vessel. It is the collisions with the walls, many millions of them every second, that are experienced as the pressure of the gas, like the pressure of hailstones hitting the ground in a heavy storm. The picture is not now the static one of a sponge, but the dynamic one of a collection of billard balls careering about over the surface of a table, bouncing off each other and off the sides of the table, except that the gas model is in three dimensions instead of two.

I have described these simple molecular models of gases because they illustrate one or two points which may help us to see our way through some of the logical difficulties which beset particle or wave models in quantum physics. Firstly, in the case of the gas molecules, it has been possible to decide between the two competing suggestions of Newton and Bernoulli. The dynamic model proved to be the simplest explanation of all the many other properties of gases; and it was also more fertile in making correct predictions about behaviour which had not then been observed. This double test remains the criterion by which theoretical models are judged.

But there is a second point to be noticed about this example. When a collection of billiard balls is said to be a model for a gas, there are some features of the billiard balls which are not intended to be useful features of the model.

Billiard balls are red or white, and hard and shiny, but we are not intending to suggest that gas molecules have these properties. We are, in fact, saying that gas molecules are *analogous* to billiard balls, and the relations of analogy mean that there are some properties of billiard balls which are not found in molecules. Let us call those properties, which we know belong to billiard balls and not to molecules, the *negative analogy* of the model. Motion and impact, on the other hand, are just the properties of billiard balls that we do want to ascribe to molecules in our model, and these we can call the *positive analogy*. Now the important thing about this kind of model-thinking in science is that generally speaking there will be some properties of the model about which we do not yet know whether they are positive or negative analogies, and these are the interesting properties, because they allow us to make new predictions. If gases are really like collections of billiard balls, except in regard to the known negative analogy, then from our knowledge of the mechanics of billiard balls we may be able to make new predictions about the expected behaviour of gases. Of course, the predictions may be wrong, but then we shall be led to conclude that we have the wrong model.

We are now in a position to describe the logical situation in quantum physics. What has happened is that the models of particles and waves, which are inherited from classical physics, have both thrust themselves upon our attention as obvious explanations of certain kinds of experiments in atomic physics; but when the next steps are taken to try to decide between the alternatives, and to extend one or other of them to cover new data, we find that both succeed and both fail, in a way unheard of in classical physics. When we try to describe in detail the processes going on in an experiment, for example the one with electrons passing through the two holes, we find that both models are required, and yet this seems to involve us in saying that the electrons are sometimes particles and sometimes waves, and that what they are observed to be depends entirely on the way we choose to investigate them. It is not surprising that many physicists have taken to saying that our models do not describe things as they really are, but act only as frameworks on which to hang the mathematics by which we calculate, correlate and predict the results of specified experiments.

Before we resign ourselves to the limitations imposed on our knowledge by this view, there is another point about the classical gas-models which may be helpful. We saw how the unsuccessful candidate, Newton's model, involved one feature which was not very familiar to us in our experience of ordinary objects, namely, the property of particles repelling each other at a distance. Newton's contemporaries attacked his model on the grounds that forces acting between bodies at a distance are not only unfamiliar but inconceivable, and even logically impossible. But Newton replied, and most physicists would agree with him, that he had described the laws of action of these forces in a mathematical theory, and this made his model intelligible. It was not necessary in addition to be able to imagine every feature of the model in terms of analogies with familiar objects. In other words, it should not surprise us that we have to ascribe some features to the small parts of matter which are not familiar to us from man-sized bodies. This is not a new situation in the history of physics.

What then is the problem? With regard to atomic entities, it is not just a question of unfamiliar models, although both the particle and the wave models have some unfamiliar features. In addition, there is the difficulty that both kinds of models seem to be required, and yet they contradict each other. At least four different attempts have been made to interpret the relation of the two models to the theory, namely: that *both* models are necessary, or that none is, or that the particle model alone is necessary, or that the wave model alone is.

The view that both models are necessary and are complementary to each other has already been outlined by Professor Kemmer. This 'complementarity' idea is associated with Niels Bohr, who holds that neither model can be regarded as an explanation, in the sense of pointing to mechanisms in Nature underlying phenomena, since they contradict each other, but that they can be regarded as different aspects of observable events—just as an account of what happens on the TV screen in terms of pictures of people and things is in a sense complementary to the description in terms of electron beams scanning the screen. Many philosophers of physics have felt that this complementarity argument is unsatisfactory, and some of them have preferred what Reichenbach called a restrictive interpretation. This is the 'no-model' interpretation, and it is, properly speaking, not an interpretation at all, because it asserts that nothing can be said about events in Nature which come *between* the observable phenomena, but only about the phenomena themselves. The mathematics of the quantum theory is developed in terms of states of observables, where a state of a system is the set of numbers derived by making all relevant measurements of the system. According to the *restrictive* interpretation, transitions from one state to another are fully described by the initial and final stages, and nothing further is said about intervening events. The particle and wave pictures then enter on appropriate occasions as partial representations of the mathematics, and hence only as crutches for thought and not descriptions of reality. You may recognize this restrictive interpretation, for it was the philosophic background of Professor Pippard's talk, and here I think most physicists would agree with him. The third view, represented by Max Born, regards the particles as fundamental, and the waves as mathematical devices which measure the probability that a particle will be found at a given place. Yet a fourth view, represented by Schrödinger, holds that the waves are fundamental. Some of the reasons why these last, one-sided views have not been generally adopted have been indicated in the previous talks.

In this situation it may seem that the obvious conclusion is that both the particle and the wave models are incorrect, and that we must look for a third kind of model which will enable us to ask detailed questions about such things as the path of an electron through the holes. But this turns out to be much more difficult than one would expect, and there are even arguments claiming to show that it is in principle impossible to go behind the partial models of modern wave mechanics to a third and quite distinct type of model. Just because particles and waves are so firmly entrenched and successful in classical physics, and because our apparatus consists of relatively large bodies and is described in terms of classical physics, it is remarkably difficult to break out of this accustomed framework of concepts. The suggestion is not quite hopeless, however, and we shall hear more about it from Professor Pryce and Professor Bohm in the discussion which follows.

Meanwhile, physicists find themselves compelled to go on talking in terms of particle and wave models, and this is, I think, not only a concession to the novice or the non-mathematical layman, but essential in the actual context of research, as a glance at any journal of theoretical physics will show. And yet this seems very unsatisfactory because, if we are after all forced to take both models seriously in some sense, the duality between them offends against a deep-seated conviction that reality is ultimately one, and that there must be a unity in the fundamental concepts of our theories. Let me try to justify the assertion that the models are still taken seriously, whatever physicists may say about them. Let me first ask another question: What do we mean by saying that a model is, or is not, a description of reality? What, in fact, do we mean when we say we can distinguish between reality and illusion in any circumstances; for example, between a real oasis and a mirage? Surely we mean that we can make further investigations, and if we then have experiences which are associated with oases—walking up to the shade of palm trees and quenching our thirst in the water, and so on—we conclude that what we saw some distance away across the desert was indeed a real oasis and not an illusion. Now this is exactly what we do with our suggested models—we try to investigate the further properties we expect them to have, and if we are correct in our expectations we shall conclude that the model, at least as far as we have gone, is a description of reality and not an illusion arising from our previous partial knowledge. Professor Pippard gave a good example. One of the things that satisfied him about the wave description was the natural explanation it gave of surprising facts about the emission of an alpha-particle from a heavy nucleus. It is correlations like this which make Professor Kemmer say that in some examples 'the wave is the true underlying reality and is physically something far more concrete than just a way of describing a probability distribution'. What criteria for reality could we have in physics other than the satisfaction of our expectations for objects of a certain type, whether particles or waves? Of course the trouble is that in quantum physics neither the particle nor the wave model alone satisfies *all* our expectations. On the other hand, if we were forbidden to talk in terms of models at all, we should have no expectations at all, and we should then be imprisoned for ever inside the range of our existing experiments. But this is not so. Physicists handle new situations with confidence, and are sometimes able to make quite spectacular predictions, such as the existence and properties of new fundamental particles—the positron or meson, for example—which are afterwards found to be 'real', by which we mean that they have effects which we can predict and then confirm by observation.

In the terminology I introduced earlier, we can put it like this: the particle model has some positive analogy with atomic entities and some negative analogy, and the same applies to the wave model. Much of the particle model's positive analogy is the wave model's negative analogy, and vice versa, and this is why the two models appear to be contradictory. If that were all there were to say, we would simply make mathematical abstractions from the two sorts of positive analogies and drop all the talk about particles and waves, but that is not all there is to say, because in both cases there are still features which we cannot yet classify as positive or negative analogies. And it is in arguing in terms of these features that the particle and wave models are still essential, supplemented by the hunches that physicists have acquired about when to argue in terms of one and when the other. The particle and wave models themselves cannot be regarded as simply descriptive of reality, but when taken together in this complicated way they can be regarded as giving us knowledge of the real world. The principles of Nature need not, as Joseph Glanville put it in 1661, be confined to our shallow models, but our knowledge need not be confined to them either. Perhaps the physicist need not be quite so modest after all.

4

A Discussion

4

A Discussion

BETWEEN

PROFESSOR MAURICE PRYCE, F.R.S.
of the Department of Physics,
University of Bristol

AND

PROFESSOR DAVID BOHM
of Birkbeck College,
University of London

PRYCE: In this discussion I seem to stand for orthodoxy and you for unorthodoxy—that's fair, isn't it? For instance, you'd criticize the orthodox viewpoint on the ground that it doesn't give enough prominence to actuality in physical theory. Is that right?

BOHM: That's my point of view, yes. I think I could give an example which would help to clarify the reason why I'm opposed to the usual point of view. It is once more, basically, the interference experiment with two slits. We begin with an electron gun, for example; accelerate electrons into a well-defined beam; pass them through a pair of slits fairly close together; and then detect them. One way of detecting electrons is with a photographic plate. Now, each electron leaves, as we all know, a track, a well-defined track, on this plate. Let us suppose that we send in the electrons essentially one at a time. Let us say, for example, one per second. We know that each electron will leave one track as it arrives. A second later another electron comes and leaves another track. Eventually a large number of tracks accumulate on the plate and we get the well-known interference pattern which has been discussed earlier. As Professor Pippard said, there is no simple way to understand how the electron manages to produce a single track and yet show interference; that is, if two slits are open, the electron cannot arrive at certain places where it could arrive if only one slit were open. Let us now replace the single photographic plate by a moving-picture camera which takes a hundred pictures per second. Since there's only one electron per second coming along, it's clear that every frame will essentially receive either one electron or none.

PRYCE: These electrons are coming out at random from their source; it's an averaged effect?

BOHM: An averaged effect, yes. Otherwise nothing is known about it. Now, after the films are developed, a scientist starts to look at them. He knows from the theory that it is possible that an electron may strike a particular plate before he has looked at it, and he may even compute the probability, even though this has no meaning for that one plate; the probability will have meaning only if he considers all the plates together and makes up the whole pattern which is the sum of the spots on all the plates. Nevertheless, everybody agrees there will be a single spot on a particular plate, an actual spot; that is, in the usual meaning of the word actuality.

PRYCE: Something that one sees when one looks down a microscope, you mean?

BOHM: That's right. One sees it right there, actually produced by certain actions of the electron. The electron functioned in the plate to produce that spot, and once the spot is there it acts; it reflects light into our eyes and so on, it does everything that actual things are supposed to do.

Pryce: So that's what you're calling actuality here; the fact that there is an actual and visible effect. I think quite a lot of people looking at this experiment, which poses quite a difficult theoretical dilemma, would ask a question like this: Which hole did the electron actually go through? Here 'actually' is used in the sense 'something that was part of the process before it got to the photographic plates'. What would you say about actuality in that context?

Bohm: That is a very relevant question. In large-scale physics—for example, with billiard balls—the answer would be quite clear. With the electron, we begin by not knowing what the electron is, and therefore we cannot answer the question, although I will try later to discuss projected or hypothetical answers.

Pryce: But would you not allow the use of the world 'actuality' in the context of saying that whatever function took place was the kind of function that either went through one slit or through the other slit? 'Actuality' in this sense is still a kind of dirty word that one must avoid in physics.

Bohm: Well, it's not that it's a dirty word, but an inappropriate word. We must use careful logic here. If we see that something has functioned, it may resemble a particle in its functioning, or it may not. Do you remember Aesop's fable about the seven blind men who encountered an elephant? One felt its trunk, and said: 'This is obviously a rope,' and another its leg, and said: 'This is clearly a tree,' and so on. Finally a man came along who could see, and he said it was one animal with all these various aspects. Now if the first blind man had tried to tie a knot in the elephant's trunk, it would not behave as ropes usually do; if the other one had tried to make logs out of the 'tree', it would not have behaved as it should. They would have gotten into a great puzzle because they were supposing the elephant to be a rope or a tree, whereas actually it is something quite different. What I want to stress is that generally speaking most experimental results in physics are really the results of functioning. We make hypotheses which suggest what may be the actual process that produced the function, and then do another experiment to test the hypotheses.

Pryce: I think I would agree with that, though I would perhaps not want to go into the philosophical description of what the theory does; I'm always much more interested in what the theory is predicting as the outcome of a particular situation. Though, again, 'prediction' here is a bad word; it's what the theory has to say about the systematization of the observations. Very often when we talk about prediction in physical theory we're really talking about post-diction. We look at something and we say on the basis of our observations 'This and that actually happened' rather than 'This and that will happen'. Don't you agree that the emphasis on prediction, which is current in discussions of the philosophy of science, is a misplaced one? Prediction is by no means the most important function of a physical theory?

Bohm: I agree with you completely on that. Basically it's one reason why I want to stress actuality; what we must do is to discuss, with the aid of theory, the actual relations that exist in the material under investigation. We don't have to predict, but unless we have a good idea of what we're talking about we cannot discuss the actual process with meaning. Now, in order to show better what I'm driving at, I will suggest a model, not a model that I necessarily believe in, but one that may help to get the idea across and that may be true. We have asked what the electron is actually doing. What might it be doing while it is passing from the source to the slit? That is the question. Well, I could propose, for example, that the electron is not a particle as we usually think of it, but a process. This process I am assuming to take place in a general medium—a 'field'. In this field we will suppose there is a pulse, a wave which moves inward and converges to a point producing a very strong pulse and then diverges and falls away. Imagine a series of such pulses one after another, all coming together to produce a series of intense pulsations on a certain line. The pulses will be so close together that the thing will look like a particle. It will act very much like a particle, too, in most cases, and yet it will act differently when it passes through a pair of small slits, because each one of the pulsations will form in a way which depends on how the whole incoming wave passes through the two slits. As a result, we have something which is neither particle nor wave. When you ask how the electron actually passed through the slit, and whether it actually went through one or the other, I would reply that the electron is, perhaps and in all probability, not the kind of thing that could pass through one slit or the other. In effect, it is actually something which is always forming and dissolving, and this may be the way in which it actually happens.

PRYCE: Well, that's fair enough, but of course it's a rather vague sort of idea. It's a vaguer interpretation of the process than one has in present physical theory. When you are being as vague as this, you have few points to which you can actually tie yourself to get further precision. And I think that precision of some kind is necessary in our descriptions. What would you say about the possibility of making a quantitative assessment of a situation, using a model or a language of that sort to describe the particular process?

Bohm: I would say first of all that it is premature to try to do this, quantitatively, with the very simple model that I proposed. This model is largely a guide to help to formulate our idea. The model can be developed further, and I have myself done so, in part, so as to put it into a more mathematical form. Let me give you an analogy: perhaps in ancient Greek times, certainly in the sixteenth century, atomic theory presented a rather vague picture of atoms moving around in a more or less complicated way, with the average results of this motion producing large-scale visible phenomena. Gradually the idea was made more precise—by applying it, for example, to the gas laws and to diffusion—until quite a bit later very precise ideas of atoms were available. Now, I think something similar has to be done here. We have to go slowly and to try to connect up to a precise experimental situation.

Pryce: But you would agree that the present quantum theory does give a quantitative description of the outcomes of experimental situations such as you have described, 'quantitative' in the sense that it predicts or specifies certain probabilities of things happening? So far as we know from having done experiments to verify the statements from the theory, the theory gives correct probabilities; but it does not give a complete description of the kind of intermediate actuality which I have asked you about. It does, of course, allow a certain amount of description of the actuality in the intermediate stages and of how the actuality enters into the theory. Isn't that one of your main dissatisfactions with the present theory?

Bohm: Yes, I think I would call that the starting-point. Everybody agrees there is a certain kind of actuality at the large-scale level with which we do our ordinary experiments. For example, a spot actually forms on the plate and the mathematics cannot discuss that fact adequately, in my opinion. Now, I think that could be taken as a starting-point; but as soon as we start we discover that we cannot stop there. You see, if the spots which we find on the plate are actual, so for that matter is the whole plate, and so is the whole context, and so are the atoms on the plate. In other words, actuality is the kind of concept which is seen to be essentially one as soon as you try to analyse it in any logical way, and therefore it leads you on immediately to generalize the idea.

Pryce: I think you would always like to be able to ask questions such as the following: When did the spot actually occur, or when did actualization of the possibilities take place? Whereas I think that there are certain questions of this kind which are grammatically acceptable questions, but meaningless in the physical context, and I suspect that this is one of them. I would prefer always to do my physics by avoiding this kind of question on the grounds that it is not a question of physics but a question of philosophy, and I would rather avoid that kind of philosophical question. I know it's a matter of taste, and you, I think, would take the opposite attitude in this.

Bohm: Yes, at one level we could say our disagreement is a matter of taste, and then the person who wants to decide between us would have to decide on the basis of *his* taste.

At another level we can each try to present such arguments as we have to back our particular choice. For I think that philosophy does enter into everything we do. Even if a physicist or any other scientist regards himself as guided purely by empirical considerations, he is none the less a philosopher, following an empiricist philosophy, whether he admits it or not. If you insist on looking merely at the facts, you cannot know whether it is better to be guided only by the facts, or whether it is better to be guided only by general opinions or ideas, or to combine these in some way or other.

PRYCE: Well, of course, I find my philosophy a little difficult, because my philosophy is to avoid philosophy. I know that even to make that statement is in itself a philosophical statement, so I'm rather in a trap here.

BOHM: Yes, that is why I said that no matter what happens you are bound to have some philosophy. It seems to me that the best thing to do is to recognize this as inevitable, to try to understand your own philosophy, to develop it and to criticize it, to allow other people to criticize it, and to see if you can't become free of those arbitrary features which are in it.

PRYCE: I think your objection to present physical theory is that it's inadequate from a philosophical point of view. You don't like some of the philosophical connotations that it has, whereas I would take completely the opposite viewpoint: that I'm quite happy with the theory because it gives what I would call precise answers to precise questions. But the degree of precision here is something which is limited by the nature, I would believe, of the physical world. There are certain questions to which I can't get an answer because, in the context of what the physical world is, they have no real meaning. And some of the questions concerning actuality are, for me, questions of that type. Now you, on the other hand, would like to say you are dissatisfied with the present theory, not because it gives false predictions in any physical situation, but because it's philosophically unsatisfying. You'd therefore like to amplify it; but, in so doing, you want to put in quite a lot of new things which at the moment one doesn't know how to verify experimentally because they seem to have no experimental or verifiable consequences. This I find unsatisfying from my point of view.

BOHM: Well, I'm not as completely satisfied with the present theory as all that. It does do a large number of things correctly, it gives statistical predictions quite well, but it does not discuss the individual process, the individual process which actually takes place, and it claims that it is not necessary to do this. But this claim is based only on the fact that it doesn't do it and that if you attempt such discussion within the present framework you cannot succeed. However, other frameworks exist in which you *can* make the attempt, and the question is: Why not try them? Thus, the notion that atoms might be important was first considered for philosophical reasons long ago. Even some physicists of the sixteenth and seventeenth centuries were guided to some extent by such ideas as that there might be an atomic constitution to matter, though many others objected that this was only speculation, philosophical speculation; that one shouldn't take it seriously and had better stick to questions of fact.

PRYCE: But, of course, it has been verified experimentally since then.

BOHM: Yes, but there always had to be a time when it wasn't verified, and somebody had to entertain the idea before it was verified.

Pryce: Fair enough.

Bohm: And now if we have a situation where people are discouraged for philosophical reasons from entertaining new ideas because they may be philosophical, this may impede the progress of physics.

Pryce: Oh, I think so too. I think everybody should be left absolutely free to criticize everything that is taught as orthodoxy. It's only a question of whether you're really going to make progress by speculating on certain lines or speculating on other lines. And this is rather a matter of taste and human ability. Some people are adepts at speculating along lines which nobody else would have explored, and a certain fraction of the major discoveries of physics have come in this way; somebody exploring something which seems to be absolutely crazy has hit upon what is the right explanation. The physical world does work in a crazy way, sometimes. On the other hand, to make that a prescription for how everybody should think about the physical world would be going too far, because then there would be chaos in the way that we look at physics.

Bohm: I don't really see that there would be chaos if everybody considered his philosophical ideas. Perhaps it's the other way round, that chaos will occur if everybody has philosophical ideas without noticing that they are philosophical. I would put it this way: that nobody should be compelled to adopt a certain philosophical idea; that would be ridiculous, and would impede the progress of science. Besides, it could have no justification whatsoever. But we must come back to this fact, that everybody does have philosophical ideas. If somebody has an idea without knowing it, and he is simply following it mechanically, then he may be limited, so that he doesn't consider new phenomena. For example, we've had this situation with parity conservation recently, where it seemed quite natural to suppose that the world is symmetrical and that parity should be conserved under reflection. But when eventually it was conceded that it might not be conserved, this disclosed a whole range of new phenomena. An important part of progress in physics is, therefore, to free ourselves from preconceptions which we are following mechanically because we don't notice where our thoughts come from.

PRYCE: Yes, it's difficult to free oneself from preconceptions. One doesn't know one's got them; one doesn't realize that they are preconceptions. This is, of course, where one has to cultivate the art of criticizing the basis of everything that one is doing. It's easy enough to do negative criticism, but it's very much more difficult to put the right new concept in the place of the old. This is where the flash of intuition is required.

BOHM: Perhaps we ought to ask ourselves what we mean by intuition, because it is commonly accepted that every now and then a brilliant intuitive flash will occur. But there are certain conditions required for intuition to operate, and one of them is to work on the problem from a number of angles so as to disclose your preconceptions and free your mind. In other words, when intuition doesn't operate it is mainly because something is preventing it from operating, namely, the previous conceptions, the 'general framework', if I may put it that way.

PRYCE: We would both agree, I think, that one of the difficulties at the moment is that a great deal of our physical theory is couched in terms of space and time, in terms of certain things happening at certain places and certain times. In the theory, these places and times are regarded as being well specified or well specifiable; but everybody would agree that this can't be satisfactory, because when we ask how we would specify a place in space, and a particular time associated with it, we can think of no physical means by which we could give it a sharp enough meaning to act as a background for the theory that we want. Yet we're so conditioned to thinking in terms of space and time that we find it extremely difficult to formulate any alternative conceptual framework.

Bohm: I'm very glad you brought this up, because it is really the key question that physics faces today. The mere application of quantum mechanics, as you say, has raised doubts whether we have a correct conception of space and time. Now, what can we do about this? That is the question that I ask myself. Here, I think, philosophy can guide us, not only in helping us to criticize our previous ideas, to know where they came from and to follow their evolution and development, but also in another way. In common experience we find reflected all kinds of possible ideas; but we tend to stress certain ones, for example the Cartesian idea of space and time in which it is assumed that every point can be measured and specified in terms of a set of continuous co-ordinates. Physics has relied on this idea for the past three centuries, but it is not actually a natural idea, nor is it the one which people had before. I want to propose another idea of space and time, the topological idea. By this I mean the study of the non-quantitative aspects of relationships in space (and, of course, in time also). For example, 'inside', 'outside', 'before', 'after', 'between', 'connected' and so on. I might mention one typical idea: How do we actually locate something in space? If we wanted to locate a glass on a table, do we give its latitude and longitude, for example, which is what the Cartesian notion would be? Obviously not. What we do is to say it is on the table, which is in the room which is in the building on the street in this city, and so on. This is a series of topological relations of being within or upon. Now, we get a whole series of such relations and that is how in common experience we locate something.

PRYCE: I agree, but the difficulty with the topological idea, at least for people who are trained with the mathematical techniques current in this part of the twentieth century, is that the mathematics is rather unfamiliar. One doesn't know so well how to put physical laws into quantitative form using topological concepts as with the more familiar algebraic and Cartesian concepts.

BOHM: I admit that, but it cannot be regarded as a criticism. In the sixteenth century people didn't know differential equations, and this was one of the main difficulties in the way of the development of classical physics. In fact, it was a gigantic achievement for Galileo to be able to describe accelerations algebraically. Until he did this, he was just as lost as everybody else in the problem of acceleration. So mathematics is one of the keys. But I do not believe topology is as difficult as people generally think it to be. What is more, I see very strong analogies between the way in which topological relations would be expressed and the way in which the laws of quantum mechanics are now expressed in terms of operators and matrices. The fact is that you can set up topological co-ordinates which are discrete; you could select 0 and 1, for instance —'zero' if it's 'outside', 'one' if it's 'inside'. Series of similar discrete numbers have been arising in quantum mechanics all the time, so there is already a certain analogy between the use of discrete specifications in quantum mechanics and the use of discrete specifications in space and time.

Pryce: That's a bit of a dangerous one, because analogy is a dangerous tool in dealing with physics. The fact that you have got discrete numbers in your topology and discrete numbers in quantum theory is no reason for thinking that they are the same or similar discrete numbers. If you could say that there are certain bigger and broader mathematical frameworks which you recognize as being essentially the same in topology and quantum theory, then I think you would be able to convince me that you were exploring a fruitful idea.

Bohm: Well, this is the problem I'm now investigating. Naturally, I'm optimistic; that is, I have been encouraged by the results so far.

Pryce: At any rate, we agree that present theory is not always adequate in some respects. In particular, it is inadequate when we come to consider what it is that keeps the nuclei of atoms together. The nuclei are, as we know, made up out of quite simple structures—protons and neutrons—and in some way the forces which keep these particles together inside the nucleus are connected with particles called mesons. But the actual way in which this connection takes place is obscure. Several variants of field theory have been put forward, but none of these is wholly satisfactory. There's something missing in our approach. We get into difficulties with mathematics yielding infinite results where quite clearly the physical situation leaves no scope for an infinity. The approach through dispersion relations, which incidentally goes a long way towards abandoning space-time ideas, is perhaps more promising. Nevertheless, the theory is obviously incomplete, and it's a question of putting this or that into it, and seeing if it's the right thing. How should we, in fact, use the theory, and how can we break loose from this conceptual framework?

Bohm: Yes, the theory has reached certain limits and, in my opinion, it's quite normal, of course, for theories to reach their limits. We must then develop new theories which are broader and more nearly correct. Apparently we have reached such a phase in the development of physics now. It is not at all clear what the next step should be; of course, it never is. For example, although the dispersion relation that you just mentioned does enable us to go a long way towards abandoning the concepts of space and time, it does not go very far towards suggesting something else which fulfils their function in the present theory; that is, to provide a principle of connection between phenomena. Up to now, I think that in trying to express such principles of connection we have been guided by common experience with space and time, refined in a certain way through mathematics. Our most common experience of space and time, as I indicated before, is topological, but if we back-track we see that at a certain stage we made an abstraction that we didn't have to make—namely, we introduced the precise Cartesian co-ordinates. In my opinion, we must now go all the way back to the beginning and make a fresh start, without bringing in the Cartesian co-ordinates.

Pryce: Well, if you want to go back to the beginning, another thing that you should consider throwing overboard is the concept of actuality.

Bohm: You may be right, but I think this concept of actuality is far more difficult to overthrow than it is to change the concept of Cartesian co-ordinates. All this will require a revolution in thinking which will make quantum mechanics look like a minor change. If you will only look into the way we actually use our (I cannot get away from the word) actuality space and time, you must admit that this notion is very, very general; it appears in every part of mathematics and physics so far. Some day in the future we may not use actuality—nobody knows what the future will bring, of course. But we have not yet exhausted what can be learned by thinking about the meaning of the word 'actuality'.

PRYCE: Nor have we exhausted what we can do with the quantum mechanics of fields in their present context.

BOHM: Except that we have discovered contradictions and inadequacies so that nobody knows quite how to proceed. I am suggesting that there should be something that cuts off the infinities due to the fact that we have points—precisely-defined mathematical points. Let's go back to the question: 'Are there any such things as precisely-defined mathematical points? Are they not pure abstractions?' Well, the answer is that obviously they're pure abstractions and they have served their purpose; we have reached their limit. If we go back to topology, from which these abstractions came, then perhaps we can make some progress. The topology must not only be the topology of space but the topology of time, because space and time are shown by relativity theory to be deeply related. If that is the case, we have no other alternative but to talk of actuality; to discuss action as the fundamental conception; to say that one thing changes and becomes another, and that this defines the process of space and time.

PRYCE: Wouldn't you say that it's not really space and time that you want to talk about now? These topological relationships are in ordinary life the relationships between objects; the space and time are, so to speak, a medium of thought that you put there to embed them in.

Bohm: I agree with you completely on that. The whole direction of my thinking is to recognize this and to free myself from this medium of thought. I say that the actual process which takes place is fundamental, and space and time are the means of describing the order in this process. Instead of beginning with space and time, as if they were fully in existence, and then placing various objects in it, we first begin with the whole process as it is and then try to discuss the order of things in space. But I also want to add, of course, time, since I am discussing the topology of process.

Pryce: Well, I come back to my difficulty about this. If you are going to develop a topological theory of this kind you're putting into what you're going to discuss a lot of things which are not in the present theory. Not only several more things, but infinitely many more things, are playing a part in your kind of formulation of the physical world, and these are things which somehow don't seem to come into the apprehension of what one does in the laboratory. There's so much arbitrariness in what you're doing. This I dislike.

Bohm: But there is also very much arbitrariness in what has already been put in. For example, the whole idea of co-ordinates is, to some extent, an arbitrary injection largely based on mathematical convenience. It worked, up to a point, and now we all admit that it doesn't work.

Pryce: 'Arbitrary' is the wrong word there. It's an abstraction that has been abstracted too abstractly, isn't that it? But it's not arbitrary so much as going too far in the direction of abstraction.

Bohm: And going too far in an arbitrary manner. You see, anything which goes beyond correctness is arbitrary. In so far as it is not correct, what we have added is arbitrary. So I think we must say that the very use of co-ordinates is, to some extent, arbitrary. This is the key focal point where the trouble arises at every step of mathematical calculation.

Pryce: Well, it's not arbitrary, because this is something which we find in mathematical textbooks. We know how to use it and we don't know what else we would put in its place. In that sense it's not arbitrary.

Bohm: But that conclusion is based only on our present deficiencies. Suppose, for example, that somebody had not been trained in differential equations, and therefore did not know how to solve a problem in classical physics. This would be an arbitrary restriction from one point of view; because of the inadequacies of his background, he could not solve the problem. From another point of view, of course, we could explain why he cannot solve it. But there would still be arbitrariness if he insisted that whatever he had learned before must be the correct method of solving, and that he found it too difficult to learn other forms of mathematics. If the man acted in that way, you could say there was a certain arbitrariness in his procedure.

Pryce: Yes, though I think one is still in a difficulty. Nature is behaving in certain ways that we can understand. It's also behaving in certain ways that we can't completely understand. It doesn't give us any clear-cut sign that our concepts are absolutely wrong; they're just slightly inadequate. And we're in the unfortunate situation that if we adopt your point of view of trying—so to speak—to undermine the whole conceptual framework of present-day physics, we don't know what to put in its place. We can't make any alternative prediction about what kind of phenomena to look for in which your new ideas would have verification. We would like to be in the position where Nature forces us to think in certain ways. But in the present situation Nature is not playing the game. We know that our theory is fairly inadequate, but we are not being given the vital clue. We can't tell what sharp break in our habits of thought we must make.

Bohm: Couldn't we perhaps say that Nature is not playing the game because we're not playing it either? If you have a game, you cannot expect it to be played by one side only. In other words, we have to take some active steps to look at it in another way. I don't think the situation is so difficult; there *is* something inadequate, but with certain modifications of theory we could at once enter a whole new domain of questions which can be investigated as soon as we know how to frame them.

Pryce: Well, how do you think we ought to handle this when we're talking with students in universities? Should we say to them: 'Look, all the theory that we have is really quite inadequate, and you shouldn't pay too much attention to it'? Or should one be fairly dogmatic about what the theory has achieved, and leave it to the pundits and the experts to criticize the theory?

Bohm: That is a question that I've considered a great deal, and I think there is a way out which is not so extreme. In fact, I've just encountered the problem recently in giving some courses in quantum mechanics and atomic physics to elementary students. It seems to me you could get out of the difficulty, essentially, by saying that all of our theories are in some way false, but there is a great deal of true relationships in the false theory. For example, in the sixteenth century people may have treated matter as continuous and not atomic, but a great many true relations of pressure and temperature were obtained thereby. When we discover the atomic constitution of matter, we change the idea that matter is continuous, but we do not change the relationships, at least within a certain approximation. As science develops it is continually trying to find what is the real truth in what we are saying, and where it is false. On that basis we can go forward. In any case, it is essential for us to recognize that a great deal of what we say is probably wrong, although we don't realize it.

Pryce: That's rather negative. Very often we're in the position that we realize that there's something lacking in the theory, but we don't know what it is that's lacking. Don't you think that that is a negative approach?

Bohm: Well . . .

Pryce: Confusing, rather than constructive, in discussing with people who have less experience of what it's all about.

Bohm: It needn't be. You see, it could be negative, but it needn't be. We tend too much to talk only about the solutions to our problems, and not about the problems themselves. People seem to become discouraged when they see a problem. That's their first reaction. Therefore we don't mention problems; it's more or less keeping a stiff upper lip. And then other people play on this: they would like to talk always about solutions, to show how much they know, perhaps. I think that we can admit that we have certain problems and talk about them and recognize they're there, without necessarily becoming completely discouraged by them. This is really something which ought to be changed in the teaching of physics.

PRYCE: Well, yes, I think that's right. It's much easier to give finished and completed results, finished pieces of work, rather than to come along and say: 'Look, I've been thinking about so-and-so. I think the theory is in a very unsatisfactory state, but I don't know very much what to do about it.' This is the difficulty.

BOHM: Part of the proper training of a scientist is to get used to this situation. This is inevitably what they will be meeting, at least when they graduate and start to do research. It is no use pretending to ourselves that the theory is all perfectly correct, because as soon as they begin to do research they will discover that it is not. Why delay the shock until then?

PRYCE: I think you've probably got the right idea. After all, if you've got a good man you can't keep him down, so you may as well let him know the worst at once. Then he'll be in a better position to do what we can't do.

BOHM: Yes, and maybe even the people who don't seem to have that much ability will be more ready to take the facts than one thinks. You see, there is a desire for everybody to feel that everything is all right—that's comforting. But if we simply state that we have made certain achievements and we also have certain problems I think that most people will finally accept that that's the situation as it is.

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Postscript

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Postscript

BY

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THE heresy of the past is the orthodoxy of the present. This is as true of physics as of anything else. But is it a bad thing? This is the question which I will try to answer now. At the same time, I hope to draw together some of the points already made.

Let me start with some recapitulation. As the Victorian era drew to a close, it seemed that physicists knew how to classify and handle the phenomena they studied. This was the heyday of 'classical' physics, in which the ideas of *matter* and *energy* were sharply divided into those appropriate to particles and those suitable to waves. The world seemed simple, and it all rang true. But then Nature played us a trick; for it turned out that the fundamental particles of matter 'violated' this neat classical division. Electrons showed some of the properties of waves. Phenomena, like those revealed in Professor Pippard's 'two-hole' experiment, required the marrying of ideas not quite made for each other; indeed, ideas designed to live apart.

Almost all the conceptual perplexities of quantum theory derive from this wedding of notions which physicists had sundered. They had defined *waves* and *particles* in mutually-exclusive terms. But electrons *will* behave as 'wavicles' as both Pippard and Kemmer showed; what then is the physicist to think? Miss Hesse noted that when it comes to Pippard's electrons he thinks one of four things: either that both the wave and particle models are, despite their incompatibility, simultaneously necessary for describing reality; or that neither model is necessary, both being logically dispensable; or that the particle model is ultimately correct; or that the wave model alone is.

Many practising physicists, like Bohr and Heisenberg, accept the first view—the idea of complementarity—that *both* models are necessary. Most philosophers follow Bohm and Miss Hesse in accepting the second—that neither is. The last two views have had mighty proponents. Born and Landé opt for particles alone; Schrödinger and de Broglie bank on the wave theory. But such one-sided solutions attract few physicists and fewer philosophers. So we'll concentrate on the opposition between the first two positions. The exchange between Pryce and Bohm illustrates this conflict. Both the positions are reasonable . . . that is, when stated with moderation.

Thirty years ago, when the present orthodoxy was still considered somewhat heretical, immoderate pronouncements were made about the future of physics. Indeed, von Neumann advanced a 'proof' that it would be impossible to reinstate a classical determinism in microphysics; it was logically pointless to look for 'hidden variables' underlying quantum processes. To von Neumann's proof were added the uncertainty relations, complementarity, the inevitability of statistical laws, the essential unpredictability of a particle's 'states' and the interactions of observer and phenomenon—indeed, all the novelties of quantum physics . . . and the result was set out by Niels Bohr as the distilled essence of all future microphysics. Not only had physicists to imbibe these radically new ideas, they had to abandon their old ways and old thoughts—or else be dubbed reactionary. Add to this a twist of Danish dogmatism and you have a brew which not every scientist could swallow.

Still, this was good medicine for quantum physics. The new theory, despite its unpalatable features, gave the scientists a heady power over micronature. It provided numbers—detailed quantitative descriptions—of particle populations. And nothing succeeds like success. Against this the dissenting voices of Einstein, Schrödinger and de Broglie formed but a whisper. The electron, proton and neutron fell smartly into place. The anti-electron, anti-proton and anti-neutron were soon predicted, detected and symmetrically aligned within the theory. The photon, the pi-meson and mu-meson quickly followed—but now not quite so smartly, nor in such orderly array. Recently, successes have been fewer; breaks in the ranks have appeared. As Professor Pryce remarked, the most serious of these arise when 'infinite results come out of the mathematics'.

The sort of thing he had in mind was this: when the theory's wave equations are applied to the scattering of particles of very high energy, those equations literally fall apart. The integrals of which they are made diverge. The result is an infinitude of solutions for each equation, rather than the small, tractable set of solutions which usually results at low energies. Now, equations equally well satisfied by any one of an infinity of values hardly constitute an intelligible physics at all; they are compatible with almost any experimental state of affairs whatever. Excluding nothing, they say nothing, describe nothing, predict nothing. Faced with this problem, theoreticians like Bethe, Tomanaga and Schwinger invented a technique to diminish the number of possible solutions into something which practising physicists could manage. The result is a rather arbitrary procedure called 'renormalization'. It rejects as physically unpromising most solutions of any wave equation. But it does this in a manner mathematically objectionable, physically arbitrary and aesthetically inelegant. Of course, if renormalization were *wholly* successful these shortcomings could be tolerated as easily as were the conceptual novelties of the last generation.

But this is not the case. When he is absorbed in high-energy problems the scientist must renormalize his equations in order to continue with his physics at all, but the consequences may be more costly than the gain. Before renormalization, he assumed, let us say, that a certain volume of space contained a number of particles, and the physical problem was to discover the probabilities that these particles would have certain positions, velocities or densities. But the mathematical form in which he describes his experiment is altered by renormalization, and altered in such a way that he can no longer assume that there really *are* any actual particles within the volume he is considering . . . since the equations are applicable either way, particle or not. The physicist's probability determinations then become shadowy references to the 'ghost states of particles', the references themselves are called 'negative probabilities'; and it is difficult to attach any real physical sense to these expressions. This all looks highly unpromising as physical theory.

Now, the objections raised thirty years ago, when physics was forced on to unfamiliar conceptual tracks, have again been heard through the land. The successes of quantum theory, which formerly had silenced criticisms, are no longer so obvious. Indeed, the very mathematical structure of the theory seems unsound after its failure with problems involving high-energy particles.

The range of attitudes expressed in this book corresponds to those generally adopted towards this situation and towards suggestions about what should be done. Pippard, Kemmer and Pryce view the difficulties as pointing out the need for technical repair-work inside the otherwise successful framework of quantum theory. They would frown on any wholesale rewriting of the theory as a waste of time, and would suspect anyone who aspired to do this of being driven by classical hankerings to return to the *particles* and *waves* of the nineteenth century, and to the methodology and structure of Newton's *Principia.*

Miss Hesse is not so sure of this reaction. Like other philosophers she senses some connection between the current perplexities of quantum field theory and the original decision to fuse the incompatible models. So, where physicists seek better mathematics inside the theory, philosophers and historians feel those external tremors which often precede the revolutions in the evolution of modern physics.

Enter Professor Bohm. Nor is he alone. Doctors Vigier and Feyerabend are also well-trained physicists with keen philosophical sensibilities. They have been looking for rips in the fabric of quantum theory—since for them ideas like complementarity, correspondence, uncertainty and indeterminacy have always been indefensible on philosophical grounds. They have all proposed 'hidden variable' quantum theories. This brings them into conflict with von Neumann's proof of the impossibility of such theories, and, as you might expect, they reject that proof with gusto.

The standard pattern of this controversy is that the orthodox find the Bohm-Vigier suggestions vague, speculative, shy of numbers and hence an object of scorn. These heretics are stigmatized as 'mere philosophers'. This is the ultimate insult amongst physicists. The heretics tend to react sharply to what seems like legislation from senior science-dictators . . . people who, in defiance of history, presume to prescribe what will and will not count as respectable programmes of future physical research. These 'dictators' are themselves the very men who grappled with the conceptual restraints of classical physics and reinstituted a freedom of scientific thought which is reminiscent of the seventeenth century; and how it galls *them*, above all others, to be called 'dogmatic'.

Pryce and Bohm were paradigms of sobriety in their discussion, but you can take it from me that more heat than light is usually generated in this sort of exchange. You cannot blame the physicists, because the efforts of their scientific lifetimes are at stake; their conception of the goals of physics—of science itself—hangs in the balance over this issue. If Bohm, Vigier and Feyerabend are correct, then most physicists are just wrong—and the development of quantum physics since 1913 has been misguided. The broadest and deepest issues of the entire scientific enterprise are caught up in these discussions.

Very often the heretics appear to come out of these discussions mantled in rationality, glowing with philosophical insight, steeped in historical perspectives and perspicuous for their intellectual boldness. In contrast, the great ranks of orthodoxy seem reactionary and dull—adhering to the last generation's conjectures as if they were laws of thought, despite reason, philosophy and all the lessons of history. Certainly Bohm presented his case with all the eloquence we have come to expect from him. Technical physics by itself may not be able to answer him, so I should like to use his own weapons —philosophy and history—to draw up a defence for the position of most orthodox physicists who resist his proposals.

Whatever you may have thought of their discussion, I will argue that it is Professor Pryce's stand which is the more reasonable one—as well as being philosophically and historically well founded.

Miss Hesse observed that the present situation in physics is not all novel. History provides other examples of similar consternation. However, in some respects the present situation is totally unprecedented. This should be stressed lest history seem too much aligned with heresy.

When we have phenomena for which there exist two equally powerful systems of explanation, two rival theories, then the physicist is justified in using either theory as he pleases. Mere convenience might well be his only criterion for choice. But suppose that further phenomena are discovered which expose one of the two theories as weak, indeed unserviceable? Here, the practising physicist will need no prodding from philosophers or historians to adopt the remaining alternative and adapt it for future development. Any physicist who refuses to do this may well be called 'dogmatic'.

To take just one example: Newton's corpuscular theory of light predicted that light accelerated on entering a denser medium; the experiments of Fizeau and Foucault showed that it *decelerated*. A few Englishmen remained faithful to Newton even after these experimental results were published. And they were properly ostracized for being dogmatic. For the wave theory had triumphed, and there were then no good reasons for clinging to the particle theory.

Is the situation in quantum theory comparable? It is not. Are there here two equally-developed theories vying for attention: one Bohr-like and crammed with non-Newtonian novelties, and the other Bohm-like and conceptually much less perplexing? There are not. There is only one kind of theory in the field now. It is the work of many—Schrödinger, Heisenberg, Born, Dirac and von Neumann, to mention but a few; the main features of that theory were felicitously described by Pippard and Kemmer. It is a theory which is algebraically articulated, experimentally detailed and—despite renormalization—powerful in explanation. What precisely is the present alternative to this physical theory? It is a congeries of excitingly vague, bold-but-largely-formless, promising-but-as-yet-unarticulated, speculations.

Now, there is nothing whatever wrong with speculation in research. The history of physics is laced with speculation. There cannot be any good argument against men striking out on bold, promising new paths. But speculation is never an alternative to a working theory, however imperfect that theory may be. If Bohm, Vigier and Feyerabend as much as hint that they have an available alternative theory they do themselves a disservice. Until their speculations churn out *numbers* it is highly reasonable to doubt that they ever will. That's insurance!

The history of physics is a history of a few workable theories which managed to float clear of seas of speculation. At almost every turn the alternative to the current working theory was—nothing. Or at least nothing intelligible. For example, classical celestial mechanics was beginning to fall apart in 1850 when Leverrier discovered the anomalous behaviour of the planet Mercury—a phenomenon which was not really explained until 1916. But no one in 1850 proposed dropping celestial mechanics, or even rewriting it—although speculation was no less in evidence then. Since there was no genuine alternative to Newtonian theory, dropping that theory then would have been to stop doing planetary astronomy altogether. And dropping orthodox quantum theory right now would be to stop doing microphysics altogether.

But make no mistake about it—should the heretics succeed in accounting for everything which orthodox quantum theory now describes, and should they do so without the divergence difficulties and without the nuisance of renormalization—and even without the uncertainty relations and irreducibly statistical laws, to complete thepicture—should they succeed in all this, the physicists of this world will be at their feet; science will have ascended to a new plane of power and fertility.

But there is now no good reason for thinking that they will succeed. Hence it is not unreasonable to assume they will *not* succeed. This implies no constraint against their continuing to try to succeed, or against their speculating *ad infinitum*. But the popular picture of the situation needs retouching. This is not a case of the suppression of a new theory by a dictatorial orthodoxy—nor are philosophically profound physical ideas being held at bay by the hard-headed naïveté of Synchrotron operators.

Quantum theory needs algebraic help, not philosophical speculation alone. No scientist worth his salt would refuse such help from Bohm or Vigier—whatever its motivation. But until help of that kind is forthcoming, the motivation by itself will not, and should not, animate all physicists.

So it is reasonable for Bohm, Vigier and Feyerabend to continue to speculate—and unreasonable for any practising physicist to dissuade them from doing so on the quite specious grounds that quantum mechanics is a beautiful success as it now stands. But it is also reasonable for practising physicists to act on the assumption that Bohm, Vigier and Feyerabend will not succeed with the development of their speculations, and unreasonable for anybody to interpret this lack of confidence as a symptom of dogmatic orthodoxy.

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